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THE MONUMENT TO GARIBALDI.

By Emilio Galloni. Inaugurated in Rome, September 20, 1895.

THE MONUMENT TO GARIBALDI IN ROME.

THE fêtes celebrating the twenty-fifth anniversary of the entry of the Italian army into Rome began on September 14, and culminated on September 20, in the ceremony of the unveiling of the monument of General Garibaldi on the Janiculum Hill in Rome in the presence of King Humbert, the royal family, the court and deputations of veterans who served under Garibaldi. At least fifty-thousand people witnessed the ceremony and they displayed the utmost enthusiasm.

Rome is always very gayly decorated on September 20, but on this occasion the decorations of public and private buildings were of the most imposing and liberal character. The English and American embassies were appropriately decorated. The equestrian statue of Garibaldi, which we illustrate, is by the Tuscan sculptor Emilio Gallori. It is of bronze, of heroic size, and weighs fifteen tons. Garibaldi is attired in the shirt, cap and poncho of tradition. His seat in the saddle is a graceful and easy one. It has been erected on the grounds of the Villa Corsini upon an immense granite base, on the four sides of which are allegorical groups.

The side facing Rome represents the defense of Rome against the French in 1848, the opposite shows the Garibaldians taking Calata Fima, the third and fourth sides exhibit groups symbolical of Europe and America. The work is described as a powerful piece of conventional art. Premier Crispi delivered an oration on this occasion which eulogized Garibaldi without saying anything that might give offense to either the Quirinal or Vatican. The procession of Garibaldian Veterans to the Villa Corsini was an impressive spectacle as they marched through the streets in their red shirts. When the veil which covered the statue was withdrawn the Garibaldians stormed and climbed the monument to deposit upon it flags and crowns of flowers. In the afternoon the mayor of Rome unveiled the Memorial Column at the Porta Pia, which is erected upon the exact spot on which the Italian troops effected a breach in the city walls when they occupied Rome in 1870 and overthrew the temporal power of the Pope.

Guiseppe Garibaldi was born at Nice, which was then a part of Italy, on July 4, 1807. He was a celebrated Italian patriot. He was exiled from Italy for political reasons in 1834 and then went to South America, where he was employed in the service of the Republic of Rio Grande do Sul and afterward in that of Uruguay. In 1849 he took service under the Roman Republic; it was abolished in the same year, and in 1850 he came to the United States, where he became naturalized. He followed the occupation of a candle maker at Clifton, Staten Island, in a small white house which may still be seen. He returned to Italy in 1854 and settled on the island of Caprera. He was engaged in many of the insurrections in Italy against the authority of Austria.

In 1860 he descended upon Sicily with a thousand volunteers, and, after having made himself dictator, he crossed to the mainland, where he expelled Francis II from Naples and entered the capital on September 7, 1860. On the union of the two Sicilies and the proclamation of Victor Emmanuel of Sardinia as King of Italy, he again retired to his island home at Caprera, relinquishing all his conquests to Victor Emmanuel.

He organized other expeditions and was actively engaged in the liberation of Italy, until on September 20, 1870, Rome was taken and United Italy became an accomplished fact.

His later years were passed at Caprera, an island near Sardinia, where he died June 2, 1882. His funeral, on June 11, 1882, was of a national character and will be found described in the SCIENTIFIC AMERICAN SUPPLEMENT, No. 343. For our engraving of the new monument we are indebted to L'Illustrazione Italiana.

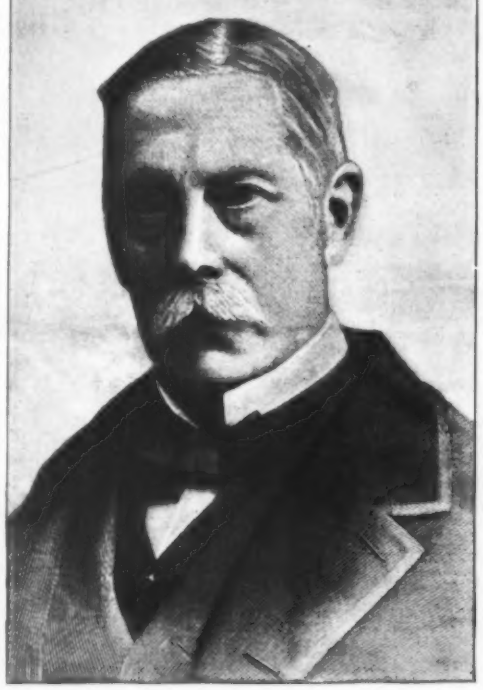
THE BRITISH ASSOCIATION.

SCIENTIFIC PROGRESS IN SIXTY-FIVE YEARS.

THE meeting of the British Association was inaugurated on Wednesday, September 11, at Ipswich, when, in presence of a large audience, Sir Douglas Galton, the president, began the delivery of his opening address. After a warm tribute to the memory of the late Mr. Huxley, and a review of the history of the association since its foundation in 1831, he mentioned some of the various directions in which it had contributed to the progress of science, such as the attainment of greater accuracy in standards, measurements, and the construction of tools and instruments, the labors carried on in the Kew Observatory, and the improvement of methods of research in chemistry, astronomy, and the different branches of physics. The president proceeded to comment on the developments which have taken place during the past sixty years in the applications of electricity, the proclamation of the doctrine of evolution, the advances made in preventive medicine and in the study of bacteriology. He said:

"In electricity in 1831, just at the time when the British Association was founded, Faraday's splendid researches in electricity and magnetism at the Royal Institution had begun with his discovery of magneto electric induction, his investigation of the laws of electro-chemical decomposition, and of the mode of electrolytical action. But the practical application of our electrical knowledge was then limited to the use of lightning conductors for buildings and ships. Indeed, it may be said that the applications of electricity to the use of man have grown up side by side with the British Association. One of the first practical applications of Faraday's discoveries was in the deposition of metals and electro-plating, which has developed into a large branch of national industry; and the dissociating effect of the electric arc, for the reduction of ores, and in other processes, is daily obtaining a wider extension. But probably the application of electricity which is tending to produce the greatest change in our mental and even material condition is the electric telegraph and its sister, the telephone. By their agency not only do we learn, almost at the time of their occurrence, the events which are happening in distant parts of the world, but they are establishing a community of thought and feeling between all the nations of the world which is influencing their attitude toward each other, and, we may hope, may tend to weld them more and more into one family. The knowledge of electricity gained by means of its application to the telegraph largely assisted the extension of its utility in other directions. The electric light gives, in its incandescent form, a very perfect hygienic light. Where rivers are at hand the electrical transmission of power will drive railway trains and factories economically, and might enable each artisan to convert his room into a workshop, and thus assist in restoring to the laboring man some of the individuality which the factory has tended to destroy. In 1843 Joule described his experiments for determining the mechanical equivalent of heat. But it was not until the meeting at Oxford, in 1847, that he fully developed the law of the conservation of energy, which, in conjunction with Newton's law of the conservation of momentum, and Dalton's law of the conservation of chemical elements, constitutes a complete mechanical foundation for physical science. Who, at the foundation of the association, would have believed some far seeing philosopher if he had foretold that the spectroscope would analyze the constituents of the sun and measure the motions of the stars; that we should liquefy air and utilize temperatures approaching to the absolute zero for experimental research; that, like the magician in the "Arabian Nights," we should annihilate distance by means of the electric telegraph and the telephone; that we should illuminate our largest buildings instantaneously, with the clearness of day, by means of the electric current; that by the electric transmission of power we should be able to utilize the falls of Niagara to work factories at distant places; that we should extract metals from the crust of the earth by the same electrical agency to which, in some cases, their deposition has been attributed?

"These discoveries and their applications have been brought to their present condition by the researches of a long line of scientific explorers, such as Dalton, Joule, Maxwell, Helmholtz, Herz, Kelvin and Rayleigh,



SIR DOUGLAS GALTON, PRESIDENT OF THE BRITISH ASSOCIATION.

aided by vast strides made in mechanical skill. But what will our successors be discussing 60 years hence? How little do we yet know of the vibrations which communicate light and heat! Far as we have advanced in the application of electricity to the uses of life, we know but little even yet of its real nature. We are only on the threshold of the knowledge of molecular action, or of the constitution of the all-pervading ether. Newton, at the end of the 17th century, in his preface to the 'Principia,' says: 'I have deduced the motions of the planets by mathematical reasoning from forces; and I would that we could derive the other phenomena of nature from mechanical principles by the same mode of reasoning. For many things move me, so that I somewhat suspect that all such may depend on certain forces by which the particles of bodies, through causes not yet known, are either urged toward each other according to regular figures or are repelled and recede from each other; and, these forces being unknown, philosophers have hitherto made their attempts on nature in vain.' In 1848 Faraday remarked: 'How rapidly the knowledge of molecular forces grows upon us, and how strikingly every investigation tends to develop more and more their importance! A few years ago magnetism was an occult force, affecting only a few bodies; now it is found to influence all bodies, and to possess the most intimate relation with electricity, heat, chemical action, light, crystallization, and through it the forces concerned in cohesion. We may feel encouraged to continuous labors, hoping to bring it into a bond of union with gravity itself.' But it is only within the last few years that we have begun to realize that electricity is closely connected with the vibrations which cause heat and light, and which seem to pervade all space—vibrations which may be termed the voice of the Creator calling to each atom and to each cell of protoplasm to fall into its ordained position, each, as it were, a musical note in the harmonious symphony which we call the universe.

"PHYSIOLOGY: THE THRESHOLD OF EVOLUTION.

"In 1831 Cuvier, who during the previous generation had, by the collation of facts followed by careful inductive reasoning, established the plan on which each animal is constructed, was approaching the termination of his long and useful life. He died in 1832; but in 1831 Richard Owen was just commencing his anatomical investigations and his brilliant contributions to paleontology. The impulse which their labors gave to biological science was reflected in numerous reports and communications, by Owen and others, throughout the early decades of the British Association, until Darwin propounded a theory of evolution which commanded the general assent of the scientific world. For this theory was not absolutely new. But just as Cuvier had shown that each bone in the fabric of an animal affords a clew to the shape and structure of the animal, so Darwin brought harmony into scattered facts, and led us to perceive that the moulding hand of the Creator may have evolved the complicated structures of the organic world from one or more primeval cells. Richard Owen did not accept Darwin's theory of evolution, and a large section of the public contested it. I well remember the storm it produced—a storm of praise by my geological colleagues, who accepted the result of investigated facts; a storm of indignation such as that which would have burned Galileo at the stake from those who were not yet prepared to question the old authorities; but they diminish daily. We are, however, as yet only on the threshold of the doctrine of evolution. Does not each fresh investigation, even into the embryonic stage of the simpler forms of life, suggest fresh problems?

"ANTHROPOLOGY.

"The impulse given by Darwin has been fruitful in leading others to consider whether the same principle of evolution may not have governed the moral as well as the material progress of the human race. Mr. Kidd tells us that nature as interpreted by the struggle for life contains no sanction for the moral progress of the individual, and points out that if each of us were allowed by the conditions of life to follow his own inclination the average of each generation would distinctly deteriorate from that of the preceding one; but because the law of life is ceaseless and inevitable struggle and competition, ceaseless and inevitable selection and rejection, the result is necessarily ceaseless and inevitable progress. Evolution, as Sir William Flower said, is the message which biology has sent to help us on with some of the problems of human life, and Francis Galton urges that man, the foremost outcome of the awful mystery of evolution, should realize that he has the power of shaping the course of future humanity by using his intelligence to discover and expedite the changes which are necessary to adapt circumstances to man, and man to circumstances. In considering the evolution of the human race, the science of preventive medicine may afford us some indication of the direction in which to seek for social improvement. One of the early steps toward establishing that science upon a secure basis was taken in 1835 by the British Association, who urged upon the government the necessity of establishing registers of mortality showing the causes of death 'on one uniform plan in all parts of the king's dominions, as the only means by which general laws touching the influence of causes of disease and death could be satisfactorily deduced.' The general registration of births and deaths was commenced in 1838. But a mere record of death and its proximate cause is insufficient. Preventive medicine requires a knowledge of the details of the previous conditions of life and of occupation. Moreover, death is not our only or most dangerous enemy, and the main object of preventive medicine is to ward off disease. Disease of body lowers our useful energy. Disease of body or of mind may stamp its curse on succeeding generations. The anthropometric laboratory affords to the student of anthropology a means of analyzing the causes of weakness, not only in bodily, but also in mental life. Mental actions are indicated by movements and their results. Such signs are capable of record, and modern physiology has shown that bodily movements correspond to action in nerve centers, as surely as the motions of the telegraph indicator express the movements of the operator's hands in the distant office.

"BODY AND BRAIN.

"Thus there is a relation between a defective status in brain power and defects in the proportioning of the body. Defects in physiognomical details, too finely graded to be measured with instruments, may be appreciated with accuracy by the senses of the observer; and the records show that these defects are in a large degree associated with a brain status lower than the average in mental power. A report presented by one of your committees shows that about 16 per 1,000 of the elementary school population appear to be so far defective in their bodily or brain condition as to need special training to enable them to undertake the duties of life and to keep them from pauperism or crime. Many of our feeble minded children, and much disease and vice, are the outcome of inherited proclivities. Francis Galton has shown us that types of criminals which have been bred true to their kind are one of the saddest disfigurements of modern civilization; and he says that few deserve better of their country than those who determine to lead celibate lives through a reasonable conviction that their issue would probably be less fitted than the generality to play their part as citizens. These considerations point to the importance of preventing those suffering from transmissible disease, or the criminal, or the lunatic, from adding fresh sufferers to the teeming misery in our large towns. And in any case, knowing as we do the influence of environment on the development of individuals, they point to the necessity of removing those who are born with feeble minds or under conditions of moral danger from surrounding deteriorating influences. These are problems which materially affect the progress of the human race, and we may feel sure that, as we gradually approach their solution, we shall more certainly realize that the theory of evolution, which the genius of Darwin impressed on this century, is but the first step on a biological ladder which may possibly eventually lead us to understand how in the drama of

creation man has been evolved as the highest work of the creator."

Sir Douglas Galton then passed in review recent developments in mechanical engineering, metallurgy, locomotion, explosives, and the construction of guns and projectiles, and attributed much of this progress to the intercommunication among scientists and students devoted to original research which the British Association had had a large share in promoting. After referring to the extent to which in Germany reliance was placed on state aid for the promotion of science and research, Sir Douglas Galton said that hitherto in this country marvelous advances had been made by means of voluntary effort; and while the increasing field of scientific research compelled them occasionally to seek for goverment assistance, it would be unfortunate if by any change voluntary effort were fettered by state control. If students did not easily find employment, it was chiefly attributable to a want of appreciation for science in the nation at large. Every year he observed, in conclusion, the field of usefulness of the British Association was widening, and he had full confidence that its future records would chronicle a still greater progress than that already achieved, and that the British nation would maintain its leading position among the nations of the world, if it would energetically continue its voluntary efforts to promote research, supplemented by that additional help from the government which ought never to be withheld when a clear case of scientific utility had been established.

SIR DOUGLAS GALTON, PRESIDENT OF THE BRITISH ASSOCIATION.

CAPTAIN Sir Douglas Galton occupies the honorable post of president of the British Association in succession to the Marquis of Salisbury. He has justified his selection for this compliment by long and valuable services in various fields. Son of the late Mr. J. H. Galton, of Worcester, he was born in 1823. He was educated at Rugby and at the Royal Military Academy, Woolwich. Having joined the Royal Engineers, Douglas Galton soon showed his abilities. He had to do with the removal of the wrecked Royal George before he was twenty; then he served in the Mediterranean and on the Ordnance Survey. The first of a long series of secretariats began in 1848, when he acted in this capacity on a royal commission relating to the use of iron in railways. He next was appointed Inspector of Railways; in 1857 Chairman of the Commission dealing with submarine cables; and in 1862 Assistant Under-Secretary for War. But merely to recount all Captain Galton's services would be wearisome. He has had connection with commissions and congresses innumerable, while for twenty years he has been General Secretary of the British Association, over which he will soon be presiding at Ipswich. He was created C.B. 1865, and knighted in the Jubilee year. Sir Douglas has managed, by genial suavity allied to wide knowledge, to accomplish excellent work, as well as to make many friends.—Illustrated London News.

LOUIS PASTEUR.

DURING the last two years four of the giants of science have passed away. To Tyndall, Helmholtz and Huxley must now be added Louis Pasteur. In the death of the great French chemist and bacteriologist, modern science loses one of its greatest lights. It was a part of his good fortune that most of his discoveries had such an immediate practical value that his fame was more widely and popularly diffused than has been the case with many scientists whose discoveries, though perhaps equally great, have been in the domain of theory. Although he encountered many obstacles in the early part of his career, the brilliancy of his discoveries gave him enduring fame and he died full of years and honors. The chemist, the biologist, the physicist and the doctor of medicine all viewed Pasteur as the founder of some of the most important departures in their several sciences.

Pasteur died at his home near St. Cloud, France, on Saturday, September 28. For some time it had been known that his condition was serious. The increasing paralysis with which he was afflicted finally caused his death. His deathbed was surrounded by his relatives, Dr. Roux and a few other persons engaged in work at the Pasteur Institute.

Louis Pasteur was born at Dole, in the province of Jura, France, on December 27, 1822. His father was a tanner of limited means, but who nevertheless managed to give his son an excellent education, the rudiments of which he imparted himself. Louis was sent to school at Arbois and later to college at Besançon. He was received as a pupil at the Ecole Normale in 1843 and took the degree of doctor in 1847, and the next year was appointed professor in the College of Dijon and later in the University of Strasbourg. In 1854 he organized the new faculty of sciences at Lille. In 1857 he went to Paris as Scientific Director of the Ecole Normale. After this he was elected a member of the Institute and toward the end of 1863 he was appointed to the chair of geology, physical science and chemistry at the Ecole des Beaux Arts at Paris and subsequently to that of chemistry at the Sorbonne. In 1856 he received the Rumford medal from the Royal Society of London. In 1874 the government granted him a pension of 20,000 francs; in 1881 he was made a member of the French Academy. He received almost every honor that it is possible to confer upon a man of science. He possessed the grand cross of the Legion of Honor.

On the occasion of M. Pasteur's seventieth birthday a most striking illustration was afforded of the esteem in which he was held in France. The ceremony took place in the great amphitheater of the Sorbonne, which was crowded by a brilliant assembly of the foremost men of the day in science and literature. M. Pasteur entered the amphitheater leaning upon the arm of his son and upon that of the President of the Republic. All present rose to their feet and greeted the hero of the day with cheers. It is seldom that a man of science has been given such an ovation, and M. Pasteur was much affected by the touching ceremonies of this occasion.

The magnificent laboratories of the Pasteur Institute in Paris were built by popular subscriptions, and really afford the best form of monument which could be erected, and it doubly honors the great scientist in its being raised during the lifetime of the man in whose honor and for the furtherance of whose work it was designed. Dr. Paul Gibier, the head of the Pasteur Institute in America, in speaking of M. Pasteur's death, said, "Suddenly this man goes. His work will save more human lives than all the conquerors in the past and of the future did or will destroy."

The government desired to give Pasteur national obsequies and to give the body a resting place in the Pantheon, but it has been decided to inter the body in the garden of the Pasteur Institute in Paris according to his wishes. The body lay in state in the library of the Pasteur Institute and the funeral took place in Notre Dame, the cathedral of Paris.

Pasteur was of a retiring nature and never sought notoriety, so that very little is known concerning him outside of his scientific labors. His first investigations were on the nature of certain crystals and the action exerted by them on the plane of the polarization of light leading to the conclusion that all mineral or artificial products have a superposable image, and are therefore not dissymmetrical, while vegetable and animal products are atomically dissymmetrical. This dissymmetry expressing itself externally in the power of turning the plane of polarization, important results grew out of the law thus established. M. Pasteur's researches in regard to fermentation in wine and beer have been of the utmost value, and have resulted in the saving of almost an incalculable amount of money. No sooner was the difficulty with the wines overcome than he took up the investigation of the diseases of the silkworm. Pasteur, previously acquainted with the work of living ferments, was prepared to see in the vibratory corpuscles of the diseased worm the cause of the epidemic. He found that the problem of the restoration of the silk husbandry of France reduced itself to the separation of the healthful from the unhealthy moths. From this grew the parasitic theory of communicable disease to which he later gave the closest attention. During these researches the question of the germ theory of infectious disease was raised. Pasteur then began experiments which proved to be of the greatest value. His next investigations were on what was called by himself "virous attenuation." His principle was that to exhaust a soil a parasite less vigorous and destructive than the really virulent one may suffice, and that after having by means of a feebler organism exhausted the soil, without fatal results, the most highly virulent parasite will prove powerless if it be introduced into the system. This led to the now well-known method for the treatment of rabies by inoculation, by which Pasteur obtained world wide celebrity. Patients fearing hydrophobia traveled from all over Europe and America to be treated by him. Pasteur Institutes for the treatment of rabies are now found in London, Berlin, New York, and St. Petersburg.

Among his other researches are those relating to charbon, a malignant pustule, or black quarter of cattle and sheep, and his researches relating to fowl cholera. We have published a number of papers on Pasteur and his work. See the SCIENTIFIC AMERICAN SUPPLEMENT Nos. 728, 474, 897, 762, 682, 899, 335. Our illustration is from a photograph by Nadar, the great photographer and aeronaut of Paris.

LOUIS PASTEUR.

RECENT STUDIES ON DIPHTHERIA.

IT is an acknowledged fact that as regards diphtheria, personal predisposition on the part of its victims plays a most important part.

We find this well illustrated by statistics which show that it is in early childhood that the majority of cases occur, and the heaviest diphtheria death rate is recorded. Thus Feer in Basel found that the most susceptible age to diphtheria lies between the years 2 and 5 and 5 and 10; but that while the mortality among children attacked in the earlier period was 25·4 per cent., in the later period, with practically no diminution in the number of cases, the diphtheria death rate fell to 7·6 per cent. After this period there is not only a great decline in the number of cases of diphtheria, but also a marked decrease in the percentage of deaths, suggesting that with increasing age the human system is enabled gradually to develop means of protection from this terrible disease.

That some such protective power must also be possessed to a large extent by children, follows from the fact that with a disease practically endemic in some of our large cities, so many children succeed in escaping from its ravages, for it is impossible to conceive that all those who have remained unscathed have never been exposed to infection from diphtheria.

Thus Flugge has worked out an interesting diphtheria table for the city of Breslau during the years 1886–1890, in which he not only confirms Freer's observations upon the connection between age and the diphtheria death rate, but he also shows very clearly that even in the most susceptible period of child life the number of cases of diphtheria is relatively small when compared with the number of children of the same age who are not attacked.

In what does this protective power against diphtheria infection possessed by many children and a large number of adults consist? This interesting and important question Dr. Wassermann has recently endeavored to answer by making a very extensive examination of the properties possessed by the blood serum derived from patients not suffering from diphtheria, but admitted on other grounds to the Berlin Institute for Infectious Diseases. Careful inquiries were, moreover, in every case made as to the patient's previous history as regards diphtheria, and only those were included in the investigation who had never had diphtheria.

The serum which was obtained from these strangers to diphtheria was in every case tested for its immunizing or protective power by inoculating it along with a recognized lethal dose of diphtheria toxin into guinea pigs, the latter by itself having been proved capable of killing these animals without exception in from 30 to 48 hours.

The results obtained were extremely interesting. Out of seventeen children varying in age from 1¼ to 11 years, eleven yielded serum with highly protective properties as regards diphtheria, for all the animals treated with their serum and virulent diphtheria toxin experienced no ill effects whatever. Two out of the seventeen children yielded serum possessed of slightly protective power, it being found capable of delaying the death of the infected animals, while the serum de-

rived from the four remaining children had no protective properties whatever.

Among the adults the number of those yielding an anti-toxic serum was much greater, for out of thirty-four individuals the serum of as many as twenty-eight was found to be endowed with protective properties against diphtheria infection; and, as far as the investigation went, it appeared that the possession of such serum, as well as its strength or degree of efficiency, was more marked with increasing age.

That people who have gone through the ordeal of diphtheria possess such anti-toxic serum in their system has been shown by various investigators, but, so far as we know, Wassermann is the first who has proved that anti-diphtheritic serum may also be possessed by individuals who have had no previous experience of diphtheria.

This discovery serves to explain how virulent diphtheria bacilli may be present in the throat of perfectly healthy people, without producing any bad results at all. That such may be the case has been proved by most careful and trustworthy observers, and that their presence does not engender diphtheria, we must now regard as probably due to the possession of anti-diphtheritic serum by the individual who so unconsciously has harbored them. Such may also be, and probably is, the explanation of the harmless presence of virulent diphtheria bacilli in the throats of patients convalescent from diphtheria long after the disappearance of all the typical symptoms.

It does not follow, however, that because at some given time a particular individual has been found the happy possessor of anti-toxic serum, he may, therefore, rashly assume that he is forever after proof against diphtheria infection.

It must be remembered that such serum is possessed in very different degrees of strength by different individuals, and may vary also, in one and the same individual, in its protective character at different times.

Research has shown that people possessing only feebly anti-toxic serum can contract diphtheria, but in the majority of such cases it is satisfactory to learn that the symptoms are light, and the disease is mastered without much difficulty.

So far as our present knowledge goes, it would appear reasonable to admit that although the possession or non-possession of anti-toxic serum of varying degrees of strength may not be the only circumstance which regulates the fluctuating personal disposition toward diphtheria infection, that yet it may be regarded as an important factor, and Wassermann considers principal cause, in determining the apparent idiosyncrasies of diphtheria infection. What the mechanism may be whereby this anti-toxic serum is produced in the system is still a mystery; that it should be possessed by infants only eighteen months old, would incline to the belief that it is natural or inborn, and not subject to later processes of evolution.

On the other hand, however, we have the well established fact that the serum of animals which have a natural or race immunity to a particular disease is wholly devoid of power to confer protection from this disease on other classes of animals.

This remarkable circumstance has been once more very clearly demonstrated by Wassermann in the case of diphtheria, to which disease white rats are absolutely immune. In order to test the character of white rat serum as regards diphtheria infection, fatal doses of diphtheria toxin were administered to guinea pigs along with such serum, but in no single case did the latter survive, showing that this serum possessed no anti-diphtheritic properties whatever, and was incapable of protecting animals from diphtheria infection.

Thus, on the one hand, we find that natural or race immunity to a particular disease does not provide protective serum against infection from that disease in other animals, and, on the other hand, that the serum of individuals who have never had diphtheria does provide in many cases such protective serum.

Now Wassermann argues from these facts that the possession of protective human serum is not natural or born with the individual; for otherwise, as in the case of white rat serum, it would be incapable of conferring immunity, that it must therefore rather be regarded as a later acquisition, and subject to evolution processes.

In pursuing this line of reasoning, Wassermann assumes that race immunity found to be characteristic of a particular description of animal is necessarily of the same character as exceptional immunity confined to particular individuals of a race. In the one case it belongs to the whole race, while in the other it is possessed by only particularly fortunate individuals of a race.

Does not this point rather to the operation of exceptional circumstances, in which, possibly, heredity may play a part? How is it that whereas some families appear to have a faculty for contracting every zymotic disease, others exposed to similar conditions have an equally characteristic faculty for escaping such diseases?

The impression is irresistible that such a faculty is born with or natural to the individual.

It may be argued that the white rat race immunity may also be ascribed to the operation of heredity. This is quite possible, but in the one case the immunity is perfected or heredity has accomplished its work, while in the other it is incomplete and is still in an evolutionary stage. The race immunity to diphtheria, or immunity in its perfected condition, is evidently of a different order, and may also very possibly have been developed on quite different lines, from that which we have been discussing in the human subject. In what this difference consists is at present unknown, and until we have a more intimate understanding of the actual condition in the system upon which immunity depends, or a closer insight into the particular agents responsible for its production, we cannot hope to arrive at any definite conclusion.

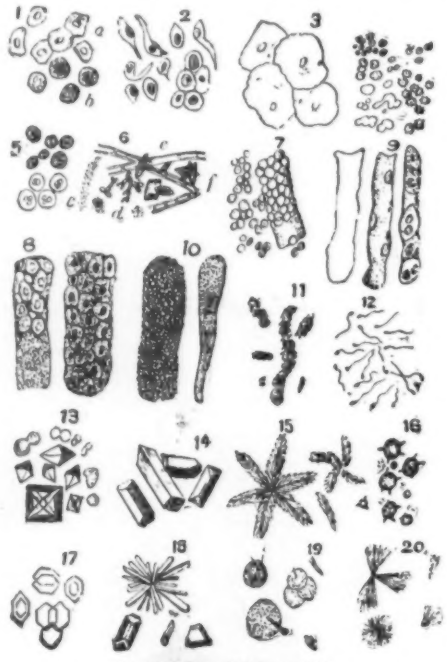
There is, however, another obstacle to a logical acceptance of Wassermann's arguments as to the origin of protective diphtheritic serum in the human system, that is to say, in the light of our present knowledge, for it entails the supposition that such individuals have been subjected to the action of diphtheria bacilli. This supposition is the logical outcome of the bacteriological evidence which is at our present command on this subject. Thus it has been found, over and over again, that the serum of animals artificially rendered immune to a particular disease is only efficacious in affording protection to other animals infected with identically the same microbial disease. This has quite recently been carefully worked out by Pfeiffer, who has shown that the serum of horses rendered immune to cholera is only efficacious in cases of infection from the cholera vibrio, and that it is absolutely inoperative in protecting from an infection due to any other vibrio, however nearly the latter may resemble that of the cholera vibrio.

But we have seen that protective serum may be possessed by individuals who have never had diphtheria, on whom, moreover, careful investigation has not been able to reveal the invariable presence of true diphtheria bacilli. So far it must be acknowledged, then, that we have no working hypothesis which enables us to comprehend aright the circumstances which determine the presence of or control the generation of anti-diphtheritic serum in the human system, and we are therefore powerless to either stimulate or diminish its production; but we are, however, in a position to regulate, to a great extent, the dissemination of diphtheria virus from one individual to another.

It has recently been shown that children taken from diphtheria surroundings, and not themselves suffering from the disease, in a large number of cases carry about with them in their nasal and throat passages typical virulent diphtheria bacilli, and that although they do not necessariiy themselves develop the disease, they thus become the dangerous carriers of infection.

It is considered essential, therefore, that no member of a family where diphtheria has occurred should be allowed to mix with others until a bacteriological examination has shown that diphtheria bacilli are absent from the air passages, neither are those who have recovered from this disease to be permitted to resume their usual occupations until the absence of diphtheria bacilli has been conclusively proved.

In Germany such systematic examinations are rapidly gaining ground, and already in some of the hygienic institutes the practice is regularly carried out. Indeed, in Konigsberg, Von Esmarch has suggested



URINARY DEPOSITS.

that to facilitate the universal adoption of such precautions, the throat of the patient or suspect should be wiped with a sterile sponge, and the latter forwarded for bacteriological examination.

The causes at present at work contributing to the generation of diphtheria in London have yet to be found.

If the contraction of diphtheria primarily depends upon the presence or absence of anti-toxic serum in the human system, then it would appear that some causes are at work tending to deprive the individual of the capacity to generate this means of protection.

It is difficult to conceive, and hard to realize, that the advance in sanitary science and improved hygienic conditions of the present day have but resulted in London in increased facilities for generating and distributing the virus of diphtheria, and that so far we have proved ourselves hopelessly unable to fathom this problem, or to stay the progress of this terrible malady.—Nature.

PREPARATIONS FOR MICROSCOPIC URINARY EXAMINATION.

To Collect a Specimen.—Take a perfectly clean four-ounce bottle well stoppered with a new, clean cork. Direct the patient to first carefully cleanse the parts, then wash the urethra by permitting a little of the urine to escape, and collect the remainder in a sterilized bowl or glass, from which it should immediately be poured into the bottle and corked. No vessel should be used which has been used for the purpose before. If the specimen is not to be used immediately, the bottle in which it is to be placed should be sterilized by either boiling in water for five or ten minutes or baking in the oven for some time. In this way, if sufficient care be taken, specimens may be kept for days or weeks without deterioration.

The best results are obtained from a mixture of the urine passed immediately after rising in the morning and that passed on retiring.

On receipt of the specimen to be examined microscopically, three or four ounces should be poured into a very clean conical glass, which should be covered with a paper cap to prevent the entrance of dust and dirt of any kind. This should then be set aside to stand undisturbed for eight or ten hours in order that any morphological constituents may subside. Even with the greatest care in collecting and handling, foreign matter, such as fibers of wool, cotton, silk, wood, etc., are likely to gain entrance; so it is of the utmost importance that the observer be familiar with the appearance of all such common objects before deciding upon the character of the urinary deposit.

With a pipette, which has been carefully cleansed and sterilized, a small drop of the deposit is placed on the center of the slide and covered with a thin glass. If too much fluid has been dropped to hold the cover glass in place, the slide should be put aside for a little while or until sufficient evaporation has taken place to permit it to be turned on its edge without slipping the cover. Care should be taken that no pressure is made upon the cover, lest the object to be studied be unduly compressed. It is really necessary sometimes, especially when the deposit contains renal casts of large size, to give support to the cover glass to prevent compression. This may be easily accomplished by placing on either side of the drop a bit of a fine hair, say a half inch in length, which will answer instead of a shallow cell, if not better. If the sediment is stained before the drop is placed on the slide, it will greatly facilitate the examination. To do this take a pipetteful of the sediment and transfer it to a small vial, add a drop or two of the aqueous solution of safranin and shake for a few moments. This stains all organized matter.

Epithelial Cells.—Although much has been said about the different kinds of urinary epithelium, there is not difference enough to say of a certainty that this flattened, tesselated form comes from the renal pelvis (Fig. 1a, healthy, b, fatty), or that that flattened or pyriform cell is from the bladder (Fig. 2), for unfortunately in practical work there is no such sharp distinction; but it is true that the superficial layers of the mucous membrane furnish epithelial cells of polygonal or elliptical shape with a single large nucleus and granular protoplasm, while those from the middle and deeper layers are of rather an oval shape, sometimes running into the long protoplasmic tails or processes having also a single large nucleus and granular protoplasm, yet all this is not absolute proof.

In diseases of the vagina and uterine cervix, the deposit is loaded with large pavement epithelial cells which are found lying in masses overlapping one another (Fig. 3). Kidney cells loaded with fat (Fig. 1b) indicates fatty degeneration in renal tissue, but a differential diagnosis cannot be founded upon these cells alone.

Red Blood Corpuscles are often found in large quantities, indicating hemorrhage. When few in number, shrunken in appearance, of a pale yellowish color looking more like washed out rings than normal red blood cells, there is ground for suspicion that the blood was effused in the kidney, pointing either to acute nephritis or tuberculosis (Fig. 4).

Pus Cells may be easily recognized with the microscope. They are about one-third larger than a red blood corpuscle and colorless. They are made up of cell walls, granular contents, and nuclei (Fig. 5). Add a drop of acetic acid, and the cell wall becomes quite transparent and the nuclei more decided. If of doubtful identity, it may be quickly dispelled by either the iodo-potassic iodide solution or Vitalis guaiacum tincture test. The former colors the cells a deep mahogany brown and the epithelium with which they are occasionally blended a light yellow; the latter gives a deep blue tint.

Casts.—There are two kinds of Casts, Unorganized and Organized. The former includes False Casts and those formed of crystals and urates. Pathologically these have but little significance.

The latter includes those derived from the loops of Henle and the collecting tubes of the kidney: (1) Blood casts, consisting of coagulated fibrin inclosing blood corpuscles (Fig. 7). They are soluble in acetic acid. (2) Epithelial casts, made up of columnar epithelium or of round cells (Fig. 8). (3) Hyaline casts are made up of a translucent, homogeneous, slightly refractive, and often barely visible, flexible, proteid material (Fig. 9). They are unaffected by acetic acid. (4) Waxy casts are made up of very refractive and brittle proteid matter. As a rule they are longer than the others, somewhat resembling a segment of the tape worm. They are so often found broken after leaving the kidney, that in the microscopic field they are seen in short fragments, notched and bearing upon their surface white and red blood corpuscles, fatty globules, arranged separately or in confluent masses, either a coating of urates or dotted with crystals of various kinds, and fungi. (5) Granular casts are dark opaque bodies composed of granular material and covered with granular cells. They differ much in character and no positive diagnosis can be formed upon them alone. They may be of all shades, from pale yellow to reddish brown. They are usually seen in fragments of various lengths and widths with well defined borders, but the bodies are variously tapered and bent (Fig. 10), they are sometimes coated with pus cells, fatty globules and crystals. (6) Fatty casts are highly refracting bodies made up of epithelial, hyaline, waxy or granular casts, filled with fatty globules.

Urea.—This is the most abundant and most important of the organic constituents of urine. To determine the presence of urea, take an inch of a small thread of cotton, dip one end into the urine and place it with its hanging drop at the center of a slide, then cover with a thin glass leaving half of the thread free. Upon this free end let fall a minute drop of nitric acid and place the slide under the microscope. The formation of the plate crystals will afford an interesting field.

Among other interesting things which urine may contain are spermatozoa (Fig. 12); tubercle bacillus (Fig. 6, e); urates; mould (Fig. 6, e); crystals of uric acid (Fig. 6, f); yeast (Fig. 6, d); oxalate of lime (Fig. 13); stellate crystals of triple phosphate (Fig. 15); ammonia magnesium phosphate (Fig. 14); acid ammonium urate (Fig. 16); cystin (Fig. 17); hippuric acid (Fig. 18); leucine (Fig. 19); tyrosin (Fig. 20); and acid sodic urate in cylinder (Fig. 11).

We are indebted to "Medical Microscopy," by J. E. Reeves, M.D., for the information and illustrations contained in this article.—Microscopical Journal.

THE PROGRESS OF SYNTHETICAL CHEMISTRY.*

IF, in my earnest desire to see the foundations of structural chemistry made more secure, I may have unwittingly given rise to the impression that I am depreciating its services as a scientific weapon, let me at once hasten to make amends by directing attention to the greatest of its triumphs, the synthesis of natural products; i. e., of compounds which are known to be produced by the vital processes of animals and plants.

Having been unable to find any recent list of the natural compounds which have become synthesized, I have compiled a set of tables which will, I hope, see the light at no very distant period. According to this census we have now realized about 180 such syntheses. The products of bacteria have been included in the list because these compounds are the results of vital activity in the same sense that alcohol is a product of the vital activity of the yeast plant. On the other hand the various uro-compounds resulting from the transformation in the animal economy of definite chemical substances administered for experimental purposes have been excluded, because I am confining my attention to natural products. Of course the importance of tracing the action of the living organism on compounds of known constitution from the physiological point of view cannot be overestimated. Such experiments will, without doubt, in time shed much light on the working of the vital laboratory.

The history of chemical synthesis has been so thoroughly dealt with from time to time that I should not have ventured to obtrude any further notice of this subject upon your patience were it not for a certain point which appeared to me of sufficient interest to merit reconsideration. It is generally stated that the formation of urea from ammonium cyanate by Wöhler in 1828 was the first synthesis of an organic compound. There can be no doubt that this discovery, which attracted much attention at the time, gave a serious blow to the current conceptions of organic chemistry, because urea was so obviously a product of the living animal. It will be found, however, that about the same time Henry Hennell, of Apothecaries' Hall, had really effected the synthesis of alcohol—that is to say, had synthesized this compound in the same sense that Wöhler had synthesized urea. The history is soon told. In 1826 Hennell (through Brande) communicated a paper to the Royal Society which appears in the Philosophical Transactions for that year.† In studying the compounds produced by the action of sulphuric acid on alcohol, and known as "oil of wine," he obtained sulphovinic acid, which had long been known, and gave fairly good analyses of this acid and of some of its salts, while expressing in the same paper very clear notions as to its chemical nature. Having satisfied himself that sulphovinic acid is a product of the action in question, he then proceeded to examine some sulphuric acid which had absorbed eighty times its volume of olefiant gas, and which had been placed at his disposal for this purpose by Michael Faraday. From this he also isolated sulphovinic acid. In another paper communicated to the Royal Society in 1828,‡ he proves quantitatively that when sulphovinic acid is distilled with sulphuric acid and water the whole of the alcohol and sulphuric acid which united to form the sulphovinic acid are recovered. In the same paper he shows that he had very clear views as to the process of etherification. Hennell's work appears to have been somewhat dimmed by the brilliancy of his contemporaries who were laboring in the same field; but it is not too much to claim for him, after the lapse of nearly seventy years, the position of one of the pioneers of chemical synthesis. Of course in his time the synthesis was not complete, because he did not start from inorganic materials. The olefiant gas used by Faraday had been obtained from coal gas or oil gas. Moreover, in 1826-28 alcohol was not generally regarded as a product of vital activity, and this is, no doubt, the reason why the discovery failed to produce the same excitement as the formation of urea. But the synthesis of alcohol from ethylene had, nevertheless, been accomplished, and this hydrocarbon occupied at that time precisely the same position as ammonium cyanate. The latter salt had not then been synthesized from inorganic materials, and the formation of urea, as Schorlemmer points out ("The Rise and Development of Organic Chemistry," p. 195), was also not a complete synthesis. The reputation of Wöhler, the illustrious friend and colleague of the more illustrious Liebig, will lose not a fraction of its brilliancy by the raising of this historical question. Science recognizes no distinction of nationality, and the future historian of synthetical chemistry will not begrudge the small niche in the temple of fame to which Hennell is entitled.

Like many other great discoveries in science, the artificial formation of natural products began, as in the case of alcohol and urea, with observations arising from experiments not primarily directed to this end. It was not till the theory of chemical structure had risen to the rank of a scientific guide that the more complicated syntheses were rendered possible by more exact methods. We justly credit structural chemistry with these triumphant achievements. In arriving at such results any defects in the theory of structure are put out of consideration because—and this point must never be lost sight of—all doubt as to the possibility of this or that atomic grouping being stable is set aside at the outset by the actual occurrence of the compound in nature. The investigator starts with the best of all assurances. From the time of Wöhler and Hennell the course of discovery in this field has gone steadily on. The announcement of a new synthesis has ceased to produce that excitement which it did in the early days when the so-called "organic" compounds were regarded as products of a special vital force. The interest among the uninitiated now rises in proportion to the technical value of the compound. The present list of 180 odd synthetical products comprises, among the latest discoveries, gentisin, the coloring matter of the gentian root (Gentiana lutea), which has been prepared by Kostanecki and Tambor, and caffeine, synthesized by Emil Fischer and Lorenz Ach, starting from dimethylurea and malonic acid.

I have allowed myself no time for those prophetic flights of the imagination which writers on this subject generally indulge in. When we know more about the structure of highly complex molecules, such as starch and albumen, we shall probably be able to synthesize these compounds. It seems to me more important just at present to come to an understanding as to what is meant by an organic synthesis. There appears to be an impression among many chemists that a synthesis is only effected when a compound is built up from simpler molecules. If the simpler molecules can be formed directly from their elements, then the synthesis is considered to be complete. Thus urea is a complete synthetical product, because we can make hydrogen cyanide from its elements; from this we can prepare a cyanate, and finally urea. In dictionaries and text books we find synthetical processes generally separated from modes of formation, and the latter in their turn kept distinct from methods of preparation. The distinction between formation and preparation is obviously a good one, because the latter has a practical significance for the investigator. But the experience gained in drawing up the tables of synthesized compounds, to which I have referred, has resulted in the conclusion that the terms "synthesis" and "mode of formation" have been either unnecessarily confused or kept distinct without sufficient reason, and that it is impossible now to draw a hard and fast line between them. Some recent writers, such, for example, as Dr. Karl Elbs, in his admirable work on this subject ("Die synthetischen Darstellungsmethoden der Kohlenstoffverbindungen," Leipzig, 1889), have expanded the meaning of the word synthesis so as to comprise generally the building up of organic molecules by the combination of carbon with carbon, without reference to the circumstance whether the compound occurs as a natural product or not. But although this definition is sufficiently wide to cover the whole field of the production of carbon compounds from less complex molecules, it is in some respects too restricted, because it excludes such well known cases as the formation of hydrogen cyanide from its elements, or of urea from ammonium cyanate. I should not consider the discussion of a mere question of terminology of sufficient importance to occupy the attention of this section were it not for a matter of principle, and that a principle of the very greatest importance, which I believe to be associated with a clear conception of chemical synthesis. The great interest of all work in this field arises from our being able, by laboratory processes, to obtain compounds which are also manufactured in nature's laboratory—the living organism. It is in this direction that our science encroaches upon biology through physiology. Now, if we confine the notion of synthesis to the building up of molecules from simpler molecules or from atoms, we exclude one of nature's methods of producing many of these very compounds which we claim to have synthesized. There can be no manner of doubt that a large proportion, if not a majority, of the natural products which have been prepared artificially are not synthesized by the animal or plant in the sense of building up at all. They are the results of the breaking down—of the degradation—of complex molecules into simpler ones. I urge, therefore, that if in the laboratory we can arrive at one of these products by decomposing a more complex molecule by means of suitable reagents, we have a perfect right to call this a synthesis, provided always that the more complex molecule, which gives us our compound, can be in its turn synthesized, by no matter how many steps, from its constituent atoms. Thus oxalic acid has been directly synthesized from carbon dioxide by Kolbe and Drechsel by passing this gas over potassium or sodium amalgam heated to 360°. Whether the plant makes oxalic acid directly out of carbon dioxide we cannot at present state; if it does, it certainly does not apply Kolbe and Drechsel's process. On the other hand, this acid may, for all that is known, exist in the plant as a product of degradation. Many more complex acids, such as citric and tartaric, break down into oxalic acid when fused with potash. Both citric and tartaric acids can now be completely synthesized; therefore, the formation of oxalic acid from these by potash fusion is a true synthesis.

The illustration given will make clear the point which I am urging. The distinction between a synthesis and a mode of formation vanishes when we can obtain a compound by the breaking down of a more complex molecule in all those cases where the latter can be completely built up. If we do not expand the meaning of synthesis so as to comprise such cases, we are simply shutting the door in nature's face. It must be borne in mind that the actual yield of the compound furnished by the laboratory process does not come into consideration, because it may be generally asserted that in most cases the artificial processes are not the same as those which go on in the animal or plant. The information of real value to the physiologist which these syntheses give is the suggestion that such or such a compound may possibly result from the degradation of this or that antecedent compound, and not from a process of building up from simpler molecules.

THE BEARING OF CHEMICAL SYNTHESIS ON VITAL CHEMISTRY.

With these views—the outcome of structural chemistry—the chemist and physiologist may join hands and move fearlessly onward toward the great mystery of vital chemistry. In considering the results of organic synthesis two questions always arise as it were spontaneously: How does nature produce these complicated molecules without the use of strong reagents and at ordinary temperatures? What bearing have our laboratory achievements on the mechanism of vitality? The light shed upon these questions by experimental investigation has as yet flickered only in fitful gleams. We are but dwellers in the outer gates, waiting for the guide who is to show us the bearing of modern research on the great problem which confronts alike the physicist, the chemist, and the biologist. The chemical processes that go on in the living organism are complex to an extent that is difficult to realize. Of the various compounds of animal or vegetable origin that have been produced synthetically some are of the nature of waste products, resulting from metabolic degradation; others are the result of zymolytic action within the organism; and others, again, are secondary products arising from the action of associated bacteria, the relationship between the bacteria and their host being as yet imperfectly understood. The answer to the question how nature produces complicated organic molecules will be much facilitated when the physiologist, by experiment and observation, shall have made possible a sound classification of these synthetical products based on their mode of origination in the organism.

The enlargement of the definition of organic synthesis which I have advocated has been rendered necessary by the consideration of certain questions which have arisen in connection with the present condition of chemical discovery in this field. What evidence is there that any one of the 180 compounds which have been prepared artificially is produced in the organism by a direct process of building up? Is not the opposite view quite as probable? May they not, from the simplest to the most complex, be products of the degradation of still more complex molecules? I venture to suggest—not without some temerity lest our colleagues of Sections I and K should treat me as an intruder—that this view should be given a fair trial. I am aware that the opposite view, especially as regards plant assimilation, has long been held, and especially since 1870, when V. Baeyer advanced his celebrated theory of the formic aldehyde origin of carbohydrates. It is but natural to consider that the formation of a complex molecule is the result of a building-up process. It must be remembered, however, that in the living organism there is always present a compound or mixture, or whatever we like to call it, of a highly complex proteid nature, which, although at present indefinite from the purely chemical point of view, is the essence of the vitality. Of course I refer to what biologists have called protoplasm. Moreover, it is perhaps necessary to state what is really nothing more than a truism, viz., that protoplasm is present in and forms a part of the organism from the very beginning of its existence—from the germ to the adult, and onward to the end of life. Any special chemical properties pertaining to protaplasm are inseparable from the animal or plant until that period arrives which Kekulé has hinted at when we shall be able to "build up the formative elements of living organisms" in the laboratory (Nature, vol. xviii, p. 212). But here I am afraid I am allowing the imagination to take a flight which I told you a few minutes ago that time would not admit of.

The view that requires pushing forward into a more prominent position than it has hitherto occupied is that all the chemical transformations in the organism—at any rate all the primary changes—are made possible only by the antecedent combination of the substances concerned with living protoplasmic materials. The carbon dioxide, water, etc., which the plant absorbs must have formed a compound or compounds with the protoplasmic material of the chloroplasts before starch, or sugar, or cellulose can be prepared. There is, on this view, no such process as the direct combination of dead molecules to build up a complex substance. Everything must pass through the vital mill. The protoplasmic molecule is vastly more complex than any of the compounds which we have hitherto succeeded in synthesizing. It might take up and form new and unstable compounds with carbon dioxide or formic aldehyde, or sugar, or anything else, and our present methods of investigation would fail to reveal the process. If this previous combination and, so to speak, vitalization of dead matter actually occurs, the appearance of starch as the first visible product of assimilation, as taught by Sachs, or the formation of a 12 carbon atom sugar as the first carbohydrate, as shown by the recent researches of Horace Brown and G. H. Morris, is no longer matter for wonderment. The chemical equations given in physiological works are too purely chemical; the physiologists have, I am afraid, credited the chemists with too much knowledge—it would appear as though their intimate familiarity with vital processes had led them to undervalue the importance of their prime agent. In giving expression to these thoughts I cannot but feel that I am treating you to the strange spectacle of a chemist pleading from the physiologists for a little more vitality in the chemical functions of living organisms. The future development of vital chemistry rests, however, with the chemist and physiologists conjointly; the isolation, identification, and analysis of the products of vital activity, which has hitherto been the task of the chemist, is only the preliminary work of physiological chemistry leading up to chemical physiology.

PROTOPLASMIC THEORY OF VITAL SYNTHESIS.

The supposition that chemical synthesis in the organism is the result of the combination of highly complex molecules with simpler molecules, and that the unstable compounds thus formed then undergo decomposition, with the formation of new products, may be provisionally called the protoplasmic theory of vital synthesis. From this standpoint many of the prevailing doctrines will have to be inverted, and the formation of the more complex molecules will be considered to precede the synthesis of the less complex. It may be urged that this view simply throws back the process of vital synthesis one stage and leaves the question of the origin of the most complex molecules still unexplained. I grant this at once; but in doing so I am simply acknowledging that we have not yet solved the enigma of life. We are in precisely the same position as is the biologist with respect to abiogenesis, or the so-called "spontaneous generation."

To avoid possible misconception, let me here state that the protoplasmic theory in no way necessitates the assumption of a special "vital force." All that is claimed is a peculiar, and at present to us mysterious, power of forming high grade chemical combinations with appropriate molecules. It is not altogether absurd to suppose that this power is a special property of nitrogen in certain forms of combination. The theory is but an extension of the views of Kuhne, Hoppe-Seyler, and others respecting the mode of action of enzymes. Neither is the view of the degradational origin of synthetical products in any way new.*

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* Abstract from the address of Professor Raphael Meldola, President of Section B, British Association, Ipswich meeting, 1895.—From Nature.

† "On the Mutual Action of Sulphuric Acid and Alcohol, with Observations on the Composition and Properties of the Resulting Compound," Phil. Trans., 1826, p. 240.

‡ "On the Mutual Action of Sulphuric Acid and Alcohol, and on the Nature of the Process by which Ether is Formed," Phil. Trans., 1828, p. 385.

* See, e. g., Vines' "Lectures on the Physiology of Plants," pp. 145, 218, 227, 233, and 234. Practically all the great classes of synthetical products

I merely have thought it desirable to push it to its extreme limit in order that chemists may realize that there is a special chemistry of protoplasmic action, while the physiologists may exercise more caution in representing vital chemical transformations by equations which are in many cases purely hypothetical or based on laboratory experiments which do not run parallel with the natural process. The chemical transformations which go on in the living organism are thus referred back to a peculiarity of protoplasmic matter, the explanation of which is bound up with the inner mechanism of the process of assimilation. If, as the protoplasmic theory implies, there must be combination of living protoplasm with appropriate compounds before synthesis is possible, then the problem resolves itself into a determination of the conditions which render such combination possible—i. e., the conditions of assimilation. It may be that here also light will come from the stereochemical hypothesis. The first step was taken when Pasteur found that organized ferments had the power of discriminating between physical isomerides; a similar selective power has been shown to reside in enzymes by the researches of Emil Fischer and his coadjutors. Fischer has quite recently expressed the view that the synthesis of sugars in the plant is preceded by the formation of a compound of carbon dioxide, or of formic aldehyde, with the protoplasmic material of the chloroplast, and similar views have been enunciated by Stohmann. The question has further been raised by Van 't Hoff, as well as by Fischer, whether a stereochemical relationship between the living and dead compounds entering into combination is not an absolutely essential condition of all assimilation. The settlement of this question cannot but lead us onward one stage toward the solution of the mystery that still surrounds the chemistry of the living organism.

RECENT DISCOVERIES OF GASEOUS ELEMENTS.

The past year has been such an eventful one in the way of startling discoveries that I must ask indulgence for trespassing a little further upon the time of the section. It was only last year at the Oxford meeting of the British Association that Lord Rayleigh and Prof. Ramsay announced the discovery of a gaseous constituent of the atmosphere which had up to that time escaped detection. The complete justification of that announcement is now before the world in the paper recently published in the Philosophical Transactions of the Royal Society. The history of this brilliant piece of work is too recent to require much recapitulation. I need only remind you how, as the result of many years' patient determinations of the density of the gases oxygen and nitrogen, Lord Rayleigh established the fact that atmospheric nitrogen was heavier than nitrogen from chemical sources, and was then led to suspect the existence of a heavier gas in the atmosphere. He set to work to isolate this substance and succeeded in doing so by the method of Cavendish. In the meantime Prof. Ramsay, quite independently, isolated the gas by removing the nitrogen by means of red hot magnesium, and the two investigators then combining their labors, followed up the subject, and have given us a memoir which will go down to posterity among the greatest achievements of an age renowned for its scientific activity.

The case in favor of argon being an element seems to be now settled by the discovery that the molecule of the gas is monatomic, as well as by the distinctness of its electric spark spectrum. The suggestion put forward soon after the discovery was announced, that the gas was an oxide of nitrogen, must have been made in complete ignorance of the methods by which it was prepared. The possibility of its being N_3 has been considered by the discoverers and rejected on very good grounds. Moreover, Peratoner and Oddo have been recently making some experiments in the laboratory of the University of Palermo with the object of examining the products of the electrolysis of hydrazoic acid and its salts. They obtained only ordinary nitrogen, not argon, and have come to the conclusion that the anhydride $N_2.N_3$ is incapable of existence, and that no allotropic form of nitrogen is given off. It has been urged that the physical evidence in support of the monatomic nature of the argon molecule, viz., the ratio of the specific heats, is capable of another interpretation—that argon is in fact an element of such extraordinary energy that its atoms cannot be separated, but are bound together as a rigid system which transmits the vibrational energy of a sound wave as motion of translation only. If this be the state of affairs, we must look to the physicists for more light. So far as chemistry is concerned, this conception introduces an entirely new set of ideas, and raises the question of the monatomic character of the mercury molecule which is in the same category with respect to the physical evidence. It seems unreasonable to invoke a special power of atomic linkage to explain the monatomic character of argon, and to refuse such a power in the case of other monatomic molecules like mercury or cadmium. The chemical inertness of argon has been referred also to this same power of self-combination of its atoms. If this explanation be adopted, it carries with it the admission that those elements of which the atoms composing the molecule are the more easily dissociated should be the more chemically active. The reverse appears to be the case if we bear in mind Victor Meyer's researches on the dissociation of the halogens, which prove that under the influence of heat the least active element, iodine, is the most easily dissociated. On the whole, the attempts to make out that argon is polyatomic by such forced hypotheses cannot at present be considered to have been successful, and the contention of the discoverers that its molecule is monatomic must be accepted as established.

In searching for a natural source of combined argon Prof. Ramsay was led to examine the gases contained in certain uranium and other minerals, and by steps which are now well known he has been able to isolate helium, a gas which was discovered by means of the spectroscope in the solar chromosphere during the eclipse of 1868 by Profs. Norman Lockyer and E. Frankland. In his address to the British Association in 1872 (Reports, 1872, p. lxxiv), the late Dr. W. B. Carpenter said:

"But when Frankland and Lockyer, seeing in the spectrum of the yellow solar prominences a certain bright line not identifiable with that of any known terrestrial flame, attribute this to a hypothetical new substance which they propose to call helium, it is obvious that their assumption rests on a far less secure foundation until it shall have received that verification which, in the case of Mr. Crookes' researches on thalium, was afforded by the actual discovery of the new metal, whose presence had been indicated to him by a line in the spectrum not attributable to any substance then known."

It must be as gratifying to Profs. Lockyer and Frankland as it is to the chemical world at large to know that helium may now be removed from the category of solar myths and enrolled among the elements of terrestrial matter. The sources, mode of isolation and properties of this gas have been described in the papers recently published by Prof. Ramsay and his colleagues. Not the least interesting fact is the occurrence of helium and argon in meteoric iron from Virginia, as announced by Prof. Ramsay in July (Nature, vol. lii, p. 234). Like argon, helium is monatomic and chemically inert so far as the present evidence goes. The conditions under which this element exists in cleveite, uraninite and the other minerals have yet to be determined.

Taking a general survey of the results thus far obtained, it seems that two representatives of a new group of monatomic elements characterized by chemical inertness have been brought to light. Their inertness obviously interposes great difficulties in the way of their further study from the chemical side; the future development of our knowledge of these elements may be looked for from the physicist and spectroscopist. Prof. Ramsay has not yet succeeded in effecting a combination between argon or helium and any of the other chemical elements. M. Moissan finds that fluorine is without action on argon. M. Berthelot claims to have brought about a combination of argon with carbon disulphide and mercury, and with "the elements of benzene . . . with the help of mercury," under the influence of the silent electric discharge. Some experiments which I made last spring with Mr. R. J. Strutt with argon and moist acetylene submitted to the electric discharge, both silent and disruptive, gave very little hope of a combination between argon and carbon being possible by this means. The coincidence of the helium yellow line with the D_3 line of the solar chromosphere has been challenged, but the recent accurate measurements of the wave length of the chromospheric line by Prof. G. E. Hale, and of the line of terrestrial helium by Mr. Crookes, leaves no doubt as to their identity. Both the solar and terrestrial lines have now been shown to be double. The isolation of helium has not only furnished another link proving community of matter, and, by inference, of origin between the earth and sun, but an extension of the work by Prof. Norman Lockyer, M. Deslandres and Mr. Crookes has resulted in the most interesting discovery that a large number of the lines in the chromospheric spectrum, as well as in certain stellar spectra, which had up to the present time found no counterparts in the spectra of terrestrial elements, can now be accounted for by the spectra of gases contained with helium in these rare minerals. The question now confronts us, Are these gases members of the same monatomic inert group as argon and helium? Whether, and by what mechanism, a monatomic gas can give a complicated spectrum is a physical question of supreme interest to chemists, and I hope that a discussion of this subject with our colleagues of Section A will be held during the present meeting. That mercury is capable under different conditions of giving a series of highly complex spectra can be seen from the memoir by J. M. Eder and E. Valenta, presented to the Imperial Academy of Sciences of Vienna in July, 1894. With respect to the position of argon and helium in the periodic system of chemical elements, it is, as Prof. Ramsay points out, premature to speculate until we are quite sure that these gases are homogeneous. It is possible that they may be mixtures of monatomic gases, and in fact the spectroscope has already given an indication that they contain some constituent in common. The question whether these gases are mixtures or not presses for an immediate answer. I will venture to suggest that an attack should be made by the method of diffusion. If argon or helium were allowed to diffuse fractionally through a long porous plug into an exhausted vessel, there might be some separation into gases of different densities, and showing modifications in their spectra, on the assumption that we are dealing with mixtures composed of molecules of different weights.

[FROM SCIENCE PROGRESS.]

CHEMICAL AFFINITY.

IN the days of prescientific chemistry the term "chemical affinity" was used to indicate the cause of chemical transformations. It was self-explanatory. Simples united to form compounds because they bore a certain resemblance to each other, and it was known that "like draws to like," so that Becker explicitly put forward the proposition that two bodies capable of combination must contain a common principle. There the matter ended.

As since advanced it was seen that the primary meaning of chemical affinity had to be given up as an explanation, but the term itself was still retained to denote the cause of chemical phenomena, of whatever nature it might eventually prove to be. Attempts were made to identify this cause with one of the known natural forces. Newton and Berthollet, for example, were of opinion that chemical force was a manifestation of gravitational attraction. That it was a form of electrical attraction found supporters in Berzelius, Davy, Grotthus and Gmelin. Williamson attempted to reduce chemical action, as well as the phenomena of heat, light and electricity, to the perpetual movements of the ultimate particles of matter.

Finally, many chemists were content to describe chemical affinity as a "special force" not further characterized. Although this position amounts to giving up the problem for the meantime, it is that which so far corresponds best with our knowledge, or want of knowledge. At present we are wholly ignorant of the cause of chemical change. All we can say is that it obeys the fundamental laws of energetics, and we know that as our knowledge of the special characteristics of chemical energy increases we shall be able to correlate it more closely with the other forms of energy, and thus in some measure arrive at an explanation of the occurrence of chemical phenomena.

A problem much better adapted to the state of science to-day is the investigation, not of the ultimate nature of chemical affinity, but of its magnitude. This problem was clearly before many chemists at the beginning of this century, and they arrived at ideas on the subject which are not yet out of date, and can bear repetition. Nicholson, in his Dictionary of Chemistry (1808), gives the following account of Berthollet's views (article "Attraction"):

"Mr. Berthollet has lately made a great number of experiments, from which he deduces the following anomaly: that in elective attractions the power exerted is not in the ratio of the affinity simply, but in a ratio compounded of the force of the affinity and the quantity of the agent; so that quantity may compensate for weaker affinity. Thus an acid which has a weaker affinity than another for a given base, if it be employed in a certain quantity, is capable of taking part of that base from the acid that has a stronger affinity for it; so that the base will be divided between them in the compound ratio of their affinity and their quantity. This division of one substance between two others, for which it has different affinities, always takes place, according to him, when three such are present under circumstances in which they can mutually act on each other. And hence it is that the force of affinity acts most powerfully when two substances first come into contact, and continues to decrease in power as either approaches the point of saturation. For the same reason it is so difficult to separate the last portions of any substance adhering to another. Hence, if the doctrine laid down by Mr. Berthollet be true to its utmost extent, it must be impossible ever to free a compound completely from any one of its constituent parts by the agency of elective attraction. . . .

"The solubility or insolubility of principles, at the temperature of any experiment, has likewise tended to mislead chemists of inferior accuracy, who have deduced consequences from the first effects of their experiments. It is evident that many separations may ensue without precipitation, because this circumstance does not take place unless the separated principle be insoluble or nearly so. The soda cannot be precipitated from a solution of sulphate of soda by the addition of potash, because of its great solubility; but, on the contrary, the new compound itself, or sulphate of potash, which is much less soluble, may fall down, if there be not enough water present to suspend it. No certain knowledge can therefore be derived from the appearance or the want of precipitation, unless the products be carefully examined."

The preceding paragraphs might almost have been copied from the latest modern text-book, and the errors against which the author warns his contemporaries still continue to mislead "chemists of inferior accuracy." The principle involved in the distribution of a base between two acids, i. e., the general principle of balanced action, affords, as we shall see, one of the best means of determining the relative affinity of the competing substances.

Some thirty or forty years ago great hopes were entertained that thermochemistry, as then understood, would furnish us with a true measure of chemical affinity. The energy lost or gained in a chemical reaction can easily be measured in terms of heat, and the determination of these heats of reaction formed almost the sole problem of thermochemistry until within quite recent years. It was thought that the chemical force or affinity causing the action must in some way be proportional to the thermal effect of the action. But this is merely a recrudescence of the old dog Latin fallacy causa æquat effectum, and has no theoretical foundation. Berthollet gave expression to this idea in his principle of maximum work, which states that in a chemical change those substances are produced whose formation occasions the greatest development of heat. Although there is no doubt that this principle roughly represents the facts when actions taking place at low temperatures are alone considered, there are so many exceptions to the rule that no general validity can be claimed for it. The chief reason for the comparative want of success of thermochemistry in the development of chemical theory is to be found in its exclusive adherence to the first law of thermodynamics, the second being altogether neglected. Of late years, however, this has been largely remedied, so that we are now in a better position to judge of the relation of heat to chemical energy.

The rate at which a chemical reaction takes place has also been proposed as a measure of the forces at work in producing the action, but this also is unsatisfactory. Sodium and chlorine are generally supposed to have a powerful affinity for each other, but if both are dry, they will not unite at the ordinary temperature. Sodium, when added to alcohol under ordinary conditions, acts on it most vigorously, especially if the alcohol contain water; Pictet, however, has shown that at −80° sodium is without action on aqueous alcohol. The actions then in these cases must go on with such extreme slowness as to be imperceptible, yet we attribute to the reacting substances considerable chemical attraction for each other.

There is, as has been said above, one class of chemical reactions which is of great service in indicating the direction in which we are to seek for an explanation of chemical affinity, which affords us a practical measure of relative affinities, and which indicates the relation of these both to velocities of reaction and to energy in general; the class, namely, of balanced actions, and it is such an action that Berthollet has under consideration when he speaks of a base being divided between two acids possessing different affinities for it. We have each of the acids acting on the salt of the other acid at a certain rate, and the point where the one action balances the other is determined by the velocities of the opposed reactions, and by the quantities of the reacting substances. Guldberg and Waage showed experimentally that the action of each substance is proportional to its active mass, i. e., to its amount divided by the volume in which it is contain-

are regarded as the results of the destructive metabolism of protoplasm. A special plea for protoplasmic action has also been urged, from the biological side, by W. T. Thiselton-Dyer, Journ. Chem. Soc., 1880: Trans., pp. 680-681.

ed, and this enables us to give an exact expression for a balanced action of any type.

Suppose that we have two acids A and A^1, with their salts S and S^1, and that A and S^1 are dissolved in such a quantity of water that their active masses are each represented by 1. Then at the beginning of the action, we have the two interacting to form the other pair A^1 and S, the chemical equation being $A + S^1 = A^1 + S$.

After equilibrium has been reached, i. e., when the two opposed reactions are balanced so that in a given time just as much of the pair A, S^1 disappears as is produced in the same time by the interaction of the pair A^1, S, a certain amount χ of the original salt and acid will have been transformed, so that their active masses are now no longer 1, but $1 - \chi$. The rate at which these pass into the other acid and base is proportional to the active mass of each, so that we have—

$$\text{rate } A, S^1 \rightarrow A^1, S = k(1 - \chi)(1 - \chi),$$

where k is a constant, expressing the amount of the reaction in unit time when the concentration of each of the reacting substances is unity. Now the active masses of the second acid and base are χ and χ respectively, for just as much of each of them has been formed as there has disappeared of the others. We have therefore—

$$\text{rate } A^1, S \rightarrow A, S^1 = k^1 \chi . \chi,$$

where k^1 is the velocity constant of the second reaction. But the amounts transformed in unit time are equal at the point of equilibrium, so that we have—

$$k(1 - \chi)^2 = k^1 \chi^2$$

$$\text{or } K = \frac{k}{k^1} = \frac{\chi^2}{(1 - \chi)^2}$$

It is therefore easy from a determination of the amounts of the various substances present at the point of equilibrium to ascertain K the ratio of the velocity constants of the opposed reactions. Julius Thomsen employed a thermochemical method for determining the extent to which the action of one acid on the salt of another had taken place.

It could be calculated from known data how much heat would be given out or absorbed when the equivalent of the first acid replaced the equivalent of the second from its salt. On actually performing the experiment the thermal effect was always found to be less than that calculated for total replacement. The ratio then of the observed to the calculated heat gave the extent to which the action had progressed. Methods similar in principle were afterward employed for ascertaining such ratios of distribution, e. g., the change of volume, and the change of the refractive index on reaction, were measured by Ostwald instead of the heat change. When the ratios obtained by balancing a series of acids against the salt of one other acid were tabulated, a set of numbers resulted which were called by Thomsen the "avidities" of the acids. These avidities were proportional to the square root of the velocity constant K. Now the constant K has an important theoretical significance, inasmuch as the maximum work to be obtained from a reversible chemical change isothermally conducted is equal to R T log. K, where R is a numerical constant and T the temperature on the absolute scale at which the equilibrium occurs. This maximum external work is the difference of the "free energy" of the two systems, if we employ the term in the sense proposed by Helmholtz, and is not proportional to the change of total energy of the two systems, i. e., to the thermal effect of the reaction. As every spontaneous natural process takes place in the direction in which external work is performed, the total amount of external work which a system can perform must have a close connection with the internal forces (in our case "chemical affinities") causing the change. Thus it has been proposed to take the change of "free energy" as a measure of the affinity instead of the change of total energy adopted by the earlier thermochemists. Little has been done as yet toward calculating affinity constants on this basis, and chemists are at present content with more empirical magnitudes. The relative affinities of the acids in particular have been well investigated from very many different points of view, and the results obtained by the various methods show excellent agreement with each other.

A method which has been found well suited for the comparison of the stronger acids with one another is found in estimating the rate at which they accelerate chemical processes. For example, cane sugar solution will remain at the ordinary temperature for an indefinite period without showing any tendency to pass into a mixture of dextrose and lævulose, but if an acid be present, the inversion of the sugar begins immediately, and goes on until the cane sugar is completely transformed. It was found that the rate of inversion with a given acid is very nearly proportional to the concentration of the acid used; but that for equivalent quantities of different acids, the rate varies greatly. If the acids are arranged in the order in which they accelerate the inversion of cane sugar, it is found that this order is identical with that obtained when the acids are arranged according to their "avidities" as determined by an equilibrium method. Other acceleration processes have now been studied, and all agree in giving the acids in the same order as they appear in the inversion of cane sugar, which order is identical with that obtained when the acids are arranged according to the electrical conductivity of their equivalent aqueous solutions.

There is no obvious connection between these two properties of acids, viz., their chemical reactivity and their power to conduct electricity, but an explanation is found in the electrolytic dissociation theory of Arrhenius. His theory refers both properties to the same cause—to the amount of dissociation of the acids into the positive ion hydrogen, and the negative ion of the acid. Acids are chemically active in the measure that they produce hydrogen ions, which alone determine the specific acid character. These hydrogen ions are also the chief carriers of electricity in an acid solution, so that the more of them an acid can produce when dissolved in a certain quantity of water, the more active will it be chemically and the better will it conduct electricity. The degree of dissociation then of an acid is an excellent measure of its strength. By applying the law of gaseous dissociation to this case of dissociation in solution, Ostwald has been able to calculate a constant from the conductivities of the weaker acids at different dilutions, which he regards as a good expression of the affinity of the acid. The dissociation constant, or affinity constant, determined in this way, is independent of the amount of water in which the acid is dissolved.

For bases we have methods corresponding to those employed for acids. First of all, we have one acceleration process—the conversion of hyoscyamine into the isomeric alkaloid atropine. Under the influence of strong bases this transformation occurs at a rate depending on the strength of the base, other things being equal. Then we have a class of reactions in which the base participates directly, namely, the saponification of ethereal salts. The rate at which ethyl acetate, for example, is converted by a base into the acetate of that base and ethyl alcohol is a measure of the strength of the base. Again, it is found that the electrical conductivity of aqueous solutions of the various bases is proportioned to their strength as determined by any of the above methods. The explanation given by the theory of electrolytic dissociation is that the specific activity of bases is due to the hydroxyl ions which they produce when dissolved in water, and that the strengths of the bases, together with their electric conductivities, are closely proportional to the number of these ions produced when the bases are in equivalent solution. A dissociation or affinity constant for bases can also be calculated in a manner precisely similar to that adopted for the dissociation constants of acids.

The proximate problem of chemical affinity, then, is to determine as many of these characteristic dissociation constants for acids and bases as possible, for they can be used in solving very many problems of the speed of reactions and the equilibrium of opposed reactions in aqueous solutions, with which the chemist is mostly concerned.

The methods detailed above are well suited for the investigation of all but the weakest acids and bases, but there they fail entirely, and we have to resort to equilibrium methods for ascertaining the affinities of those substances. There is one substance which is very convenient as a standard of comparison against which to compare the strengths of both weak acids and weak bases, namely, water. Weak bases give off comparatively few hydroxyl ions OH, and weak acids few hydrogen ions H.

Now water in the pure state dissociates electrolytically to a minute extent into these ions, $H_2O = H + OH$, so that it can act both as a very weak acid and a very weak base, according as it competes against another acid or another base. The amount of the dissociation of water is extremely slight, a calculation by Kohlrausch showing that a cubic meter, i. e., about a ton, of water at 18° C. contains only 1·64 milligrammes dissociated into hydrogen and hydroxyl ions.

A base like diphenylamine is so weak that, although it can form a hydrochloride, the salt, when treated with water, has the hydrochloric acid washed entirely away, leaving the free base behind. Here the water plays the part of a base against the diphenylamine, and competes with it for the hydrochloric acid. Aniline is a much stronger base than diphenylamine, but still its hydrochloride, when dissolved in water, is split up partially into aniline and hydrochloric acid. By comparing the amounts of free hydrochloric acid obtained from equivalent solutions of the hydrochlorides, we can ascertain the relative strengths of the bases, for the stronger the base is, the less will its salt be decomposed by a given quantity of water. The amount of free hydrochloric acid in the solutions cannot be estimated by any ordinary analytical process, but can be determined indirectly by ascertaining, for example, how rapidly the solution inverts cane sugar, the rate depending on the amount of free acid present.

Similarly salts of weak acids may be decomposed by water. The cleansing properties of washing soda and of soap depend on the fact that these are sodium salts, whose acids are so weak that there is partial, though slight, decomposition into the acid and free caustic soda when the salts are dissolved in water. The amount of soda present may be calculated from the rate at which the solution saponifies ethyl acetate.

These equilibrium methods have now put us in a position to give the order of affinity of both acids and bases from the strongest to the weakest, and even for the weakest an approximation to the dissociation constant can be calculated.

With regard to other affinities, the study of electromotive force seems to promise excellent results in certain directions, but the solution of the general problem is still the task of the future.

JAMES WALKER.

ELECTRIFICATION AND DISELECTRIFICATION OF AIR AND OTHER GASES.*

By LORD KELVIN, MAGNUS MACLEAN and ALEXANDER GALT.

§ 1. EXPERIMENTS were made for the purpose of finding an approximation to the amount of electrification communicated to air by one or more electrified needle points. The apparatus consisted of a metallic can 48 cms. high and 21 cms. in diameter, supported by paraffine blocks, and connected to one pair of quadrants of a quadrant electrometer. It had a hole at the top to admit the electrifying wire, which was 5·31 meters long, hanging vertically within a metallic guard tube. This guard tube was always metallically connected to the other pair of quadrants of the electrometer and to its case and to a metallic screen surrounding it. This prevented any external influences from sensibly affecting the electrometer, such as the working of the electric machine which stood on a shelf 5 meters above it.

§ 2. The experiment is conducted as follows: One terminal of an electric machine is connected with the guard tube and the other with the electrifying wire, which is let down so that the needle is in the center of the can. The can is temporarily connected to the case of the electrometer. The electric machine is then worked for some minutes, so as to electrify the air in the can. As soon as the machine is stopped the electrifying wire is lifted clear out of the can. The can and the quadrants in metallic connection with it are disconnected from the case of the electrometer, and the electrified air is very rapidly drawn away from the can by a blowpipe bellows arranged to suck. This releases the opposite kind of electricity from the inside of the can, and allows it to place itself in equilibrium on the outside of the can and on the insulated quadrants of the electrometer in metallic connection with it.

§ 3. We tried different lengths of time of electrification and different numbers of needles and tinsel, but we found that one needle and four minutes of electrification gave nearly maximum effect. The greatest deflection observed was 936 scale divisions. To find, from this reading, the electric density of the air in the can we took a metallic disk of 2 cms. radius, attached to a long varnished glass rod, and placed it at a distance of 1·45 cm. from another and larger metallic disk. This small air condenser was charged from the

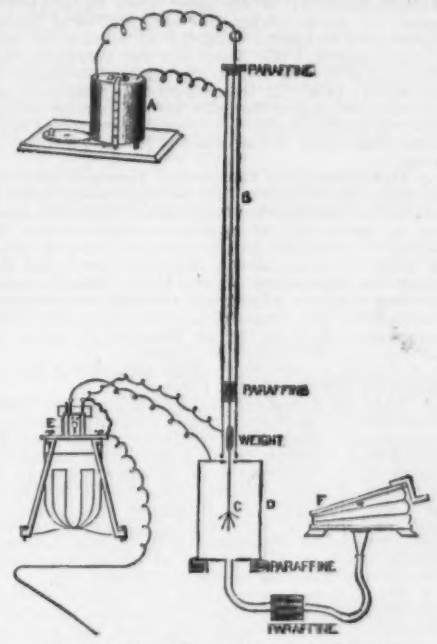


FIG. 1.

electric light conductors in the laboratory to a difference of potential amounting to 100 volts. The insulated disk thus charged was removed and laid upon the roof of the large insulated can. This addition to the metal in connection with it does not sensibly influence its electrostatic capacity. The deflection observed was 122 scale divisions. The capacity of the condenser is approximately $\frac{\pi \times 2^2}{4\pi \times 1{\cdot}45} = \frac{1}{1{\cdot}45}$. The quantity of electricity with which it was charged was $\frac{1}{1{\cdot}45} \times \frac{100}{300} = \frac{1}{4{\cdot}35}$ electrostatic unit. Hence the quantity to give 936 scale divisions was

$$\frac{1}{4{\cdot}35} \times \frac{936}{122} = 1{\cdot}7637.$$

The bellows was worked vigorously for two and a half minutes, and in that time all the electrified air would be exhausted. The capacity of the can was 16,632 cubic centimeters, which gives, for the quantity of electricity per cubic centimeter, $\frac{1{\cdot}7637}{16{,}632} = 1{\cdot}06 \times 10^{-4}$. The electrification of the air in this case was positive; it was about as great as the greatest we got, whether positive or negative, in common air when we electrified it by discharge from needle points. This is about four times the electric density which we roughly estimated as about the greatest given to the air in the

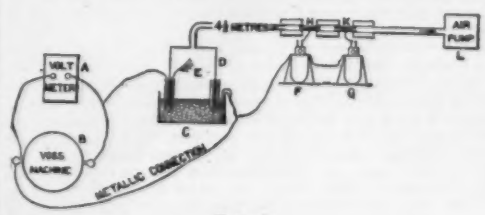


FIG. 2.

inside of a large metal vat, electrified by a needle point and then left to itself, and tested by the potential of a water dropper with its nozzle in the center of the vat, in experiments made two years ago and described in a communication to the Royal Society of date May, 1894.*

§ 4. In subsequent experiments, electrifying common air in a large gas holder over water by an insulated gas flame burning within it with a wire in the interior of the flame kept electrified by an electric machine to about 6,000 volts, whether positively or negatively, we found as much as $1{\cdot}5 \times 10^{-4}$ for the electric density of the air. Electrifying carbonic acid in the same gas holder, whether positively or negatively, by needle points, we obtained an electric density of $2{\cdot}2 \times 10^{-4}$.

§ 5. We found about the same electric density ($2{\cdot}2 \times$

* Abstract.—Communicated to Section A, Brit. Ass., September 17, 1895.

* "On the Electrification of Air," by Lord Kelvin and Magnus Maclean.

10^{-4}) of negative electricity in carbonic acid gas drawn from an iron cylinder lying horizontally, and allowed to pass by a U-tube into the gas holder without bubbling through the water. This electrification was due probably not to carbonic acid gas rushing through the stopcock of the cylinder, but to bubbling from the liquid carbonic acid in its interior, or to the formation of carbonic acid snow in the passages and its subsequent evaporation. When carbonic acid gas was drawn slowly from the liquid carbonic acid in the iron cylinder placed upright, and allowed to pass, without bubbling, through the U-tube into the gas holder over water, no electrification was found in the gas unless electricity was communicated to it from needle points.

§ 6. The electrifications of air and carbonic acid described in sections 4 and 5 were tested, and their electric densities measured by drawing by an air pump a measured quantity of the gas* from the gas holder through an India rubber tube to a receiver of known efficiency and of known capacity in connection with the electrometer. We have not yet measured how much electricity was lost in the passage through the India rubber tube. It was not probably nothing; and the electric density of the gas before leaving the gas holder was no doubt greater, though perhaps not much greater, than what it had when it reached the electric receiver.

§ 7. The efficiency of the electric receivers used was approximately determined by putting two of them in series, with a paraffine tunnel between them, and measuring by means of two quadrant electrometers the quantity of electricity which each took from a measured quantity of air drawn through them. By performing this experiment several times, with the order of the two receivers alternately reversed, we had data for calculating the proportion of the electricity taken by each receiver from the air entering it, on the assumption that the proportion taken by each receiver was the same in each case. This assumption was approximately justified by the results.

§ 8. Thus we found for the efficiencies of two different receivers respectively 0·77 and 0·31 with air electrified positively or negatively by needle points; and 0·82 and 0·42 with carbonic acid gas electrified negatively by being drawn from an iron cylinder placed on its side. Each of these receivers consisted of block tin pipe, 4 cms. long, and 1 cm. diameter, with 5 plugs of cotton wool kept in position by six disks of fine wire gauze. The great difference in their efficiency was, no doubt, due to the quantities of cotton wool being different, or differently compressed in the two.

§ 9. We have commenced and we hope to continue an investigation of the efficiency of electric receivers of various kinds, such as block tin, brass, and platinum tubes from 2 to 4 cms. long, and from 1 mm. to 1 cm. internal diameter, all of smooth bore and without any cotton wool or wire gauze filters in them; also a polished metal solid, insulated within a paraffin tunnel. This investigation, made with various quantities of air drawn through per second have already given us some interesting and surprising results, which we hope to describe after we have learned more by farther experimenting.

§ 10. In addition to our experiments on electric filters we have made many other experiments to find other means for the diselectrification of air. It might be supposed that drawing air in bubbles through water should be very effective for this purpose, but we find that this is far from being the case. We had previously found that nonelectrified air drawn in bubbles through pure water becomes negatively electrified, and through salt water positively. We now find that positively electrified air drawn through pure water, and negatively electrified air through salt water, has its electrification diminished, but not annuled, if the primitive electrification is sufficiently strong. Negatively electrified air drawn in bubbles through pure water, and positively electrified air drawn through salt water, has its electrification augmented.

§ 11. To test the effects of heat, we drew air through combustion tubes of German glass about 180 cms. long and 2½ or 1½ cms. bore, the heat being applied externally to about 120 cms. of the length. We found that when the temperature was raised to nearly a dull red heat, air, whether positively or negatively electrified, lost little or nothing of its electrification by being drawn through the tube. When the temperature was raised to a dull red heat and to a bright red high enough to soften the glass, losses up to as much as four-fifths of the whole electrification were sometimes observed, but never complete diselectrification. The results, however, were very irregular. Non-electrified air never became sensibly electrified by being drawn through the hot glass tubes in our experiments, but it gained strong positive electrification when pieces of copper foil, and negative electrification when pieces of carbon, were placed in the tube, and when the temperature was sufficient to powerfully oxidize the copper or to burn away the charcoal.

§ 12. Through the kindness of Mr. E. Matthey, we have been able to experiment with a platinum tube 1 meter long and 1 millimeter bore. It was heated either by a gas flame or an electric current. When the tube was cold and non-electrified air drawn through it, we found no signs of electrification by our receiver and electrometer. But when the tube was made red or white hot, either by gas burners applied externally or by an electric current through the metal of the tube, the previously non-electrified air drawn through it was found to be electrified strongly positive. To get complete command of the temperature, we passed a measured electric current through 20 centimeters of the platinum tube. On increasing the current till the tube began to be at a scarcely visible dull red heat we found but little electrification of the air. When the tube was a little warmer, so as to be quite visibly red hot, large electrification became manifest. Thus 60 strokes of the air pump gave 45 scale divisions on the electrometer when the tube was dull red and 395 scale divisions (7 volts) when it was a bright red (produced by a current of 36 amperes). With stronger currents, raising the tube to white hot temperature, the electrification seemed to be considerably less.

* The gas holder was 36 cms. high and 81 cms. in circumference. Ten strokes of the pump raised the water inside to a height of 8·1 cms., so that the volume of air drawn through the receivers in the experiments was 438 cubic centimeters per stroke of the pump. This agrees with the measured effective volume of the two cylinders of the pump.

ELECTRIC ELEVATORS.

Passenger elevators are now of the greatest necessity in the high buildings that are being constructed. For a long time, lifts for different kinds of merchandise sufficed. It was only in 1867, at the Universal Exposition, that Mr. Edoux exhibited the first model of a hydraulic passenger elevator. Water under pressure was at that epoch the best agent capable of raising a given weight to a certain height. All the necessary arrangements were made for assuring a proper operation of this apparatus, such as regulating devices, safety devices, balancing of a part of the weight, etc. It is certain that hydraulic elevators established under such conditions rendered the greatest services at that epoch, and have always been, since then, greatly appreciated.

Only one grave objection could be urged against them, and that concerned their establishment. They necessitated the use of a cast iron cylinder placed in a well of a depth somewhat greater than the height of the car's travel, and in which a piston was capable of moving and of wholly entering when the elevator was at the bottom of its travel. There were also constructed elevators without wells, among which may be mentioned that of Samain, with telescopic piston, that of Roux & Combaluzier, with jointed piston, etc. The various difficulties were put up with because of the advantages that elevators presented.

The maneuvers of starting and stopping were effected by acting, through a cable running through the car, upon a distributor designed to put the cistern of the piston in communication with the water conduit or the discharge pipe. Improvements were afterward made, and, in 1887, Mr. Edoux substituted a hydroelectric maneuver for that by means of a cable. It sufficed to establish a contact that closed a battery circuit upon electro-magnets in order to effect the displacing of the distributor and consequently the opening or closing of the water cock. We must add that these latter arrangements, although they sometimes operated well, also often presented numerous difficulties. Since then, however, electric elevators have often been mentioned, and we must recall here the first apparatus of Mr. Werner Siemens that figured at the exposition of Mannheim, and the electric elevators of Mr. Chretien at the exposition of 1889. But the question had not as yet received a really practical solution. As long ago as 1890, however, numerous experiments had been made in France, Germany and America with a view to substituting electric elevators for the hydraulic ones supplied by city water, or by water forced into a reservoir by a pump actuated by a gas motor. The electric elevator was set in motion by an ordinary rope windlass. We may mention, without being able to describe them, the electric elevators of Edoux, of the Société d'Eclairage Electrique, of Neu, of Otis, of the Keystone Electric Company, of Messrs. Morse, Williams & Company, etc. At the beginning of 1895 there existed at New York, upon the distributing line of one electric company, 250 elevators of a total 3,000 horse power.

Up to this epoch electric elevators were considered as interesting applications at Paris, but few owners of buildings thought of adopting them, by reason, on the one hand, of the good service that the hydraulic elevators already installed were rendering them, and, on another hand, by reason of the cost of transformation that would be necessary. A municipal regulation concerning the concession of the water of the city of Paris, published in the Official Municipal Gazette of December 22, 1894, changed all such arrangements. According to this regulation, the water used for actuating the engines by means of the pressure that it possesses in the public pipe line had to be paid for apart and at the rate of 0·6 franc per cubic meter of water consumed, conformably to the readings of a meter through which it was to pass isolatedly. The cost of the water consumed was thus doubled, and the expenses increased in large proportions, and the more so in that the consumption of hydraulic elevators is constant, whatever be the number of passengers, and even when empty. It has been calculated that a three-passenger elevator making, upon an average, twenty-five trips a day at the rate of 275 liters of water a trip, would cost 1,500 instead of 800 francs a year at the old tariff of 0·32 franc per cubic meter of water. All the manufacturers of elevators became stirred up and endeavored to modify their systems in order to remedy this state of things. Afterward an examination was made of electric, compressed air and what are called hydro-electric elevators. Under several circumstances, the project was likewise examined of preserving the present hydraulic elevators and of supplying them with water under pressure pumped by gas or electric motors. These latter arrangements are evidently

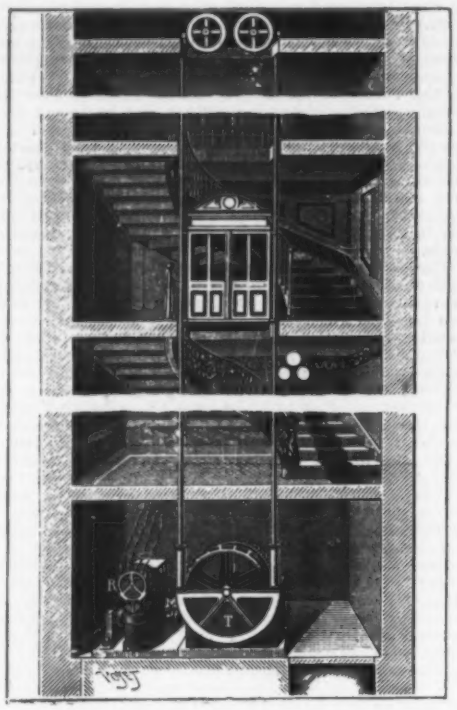


Fig. 1.—GENERAL VIEW OF AN ELECTRIC ELEVATOR.

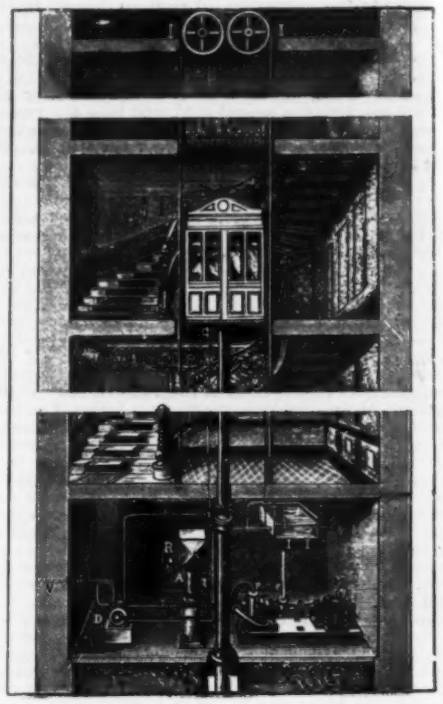


Fig. 2.—GENERAL VIEW OF A HYDRO-ELECTRIC ELEVATOR

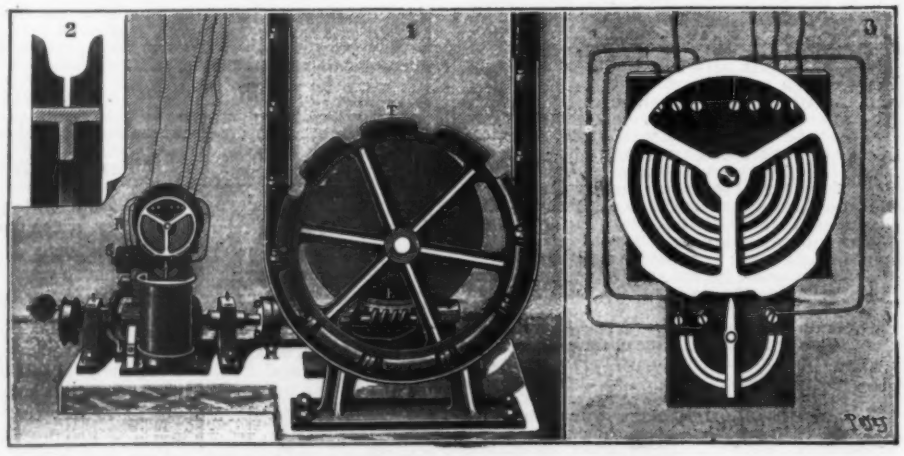


Fig. 3.—DETAILS OF AN ELECTRIC WINDLASS FOR AN ELEVATOR.

1. Electric control. 2. Section of a notch. 3. Rheostat.

adapted only to certain houses that are provided with several elevators and are capable of paying the cost of the first establishment.

The installations of electric elevators are not yet numerous in Paris, but we have recently had an opportunity of inspecting two elevators established by the Société d'Eclairage Electrique, which has undertaken the transformation of the hydraulic elevators of the principal manufacturers: Messrs. Edoux, Roux & Combaluzier, Pitre, Heurtebize, etc. We are going to make known the principal arrangements adopted by this society, and shall afterward set forth some comparative net costs.

The systems employed are of two sorts: electric and hydro-electric elevators. The first includes a radical

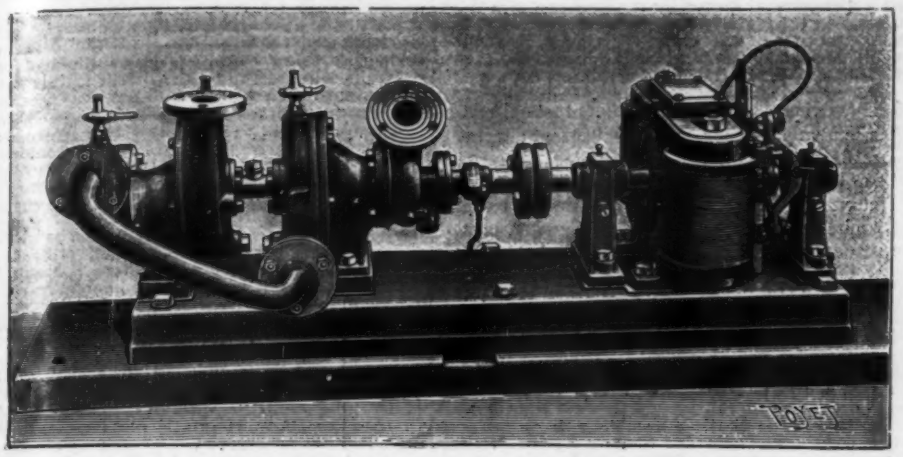


FIG 4.—GENERAL VIEW OF A MAGINOT-PINETTE PUMP, ACTUATED BY AN ELECTRIC MOTOR AND USED IN A HYDRO-ELECTRIC ELEVATOR.

modification of the present elevators, while the second, on the contrary, requires but a few modifications of no importance. The present electric elevators employ continuous currents only, but the question of alternating current electric elevators is under study and will soon receive a satisfactory solution.

The elevator with electric windlass consists essentially (Fig. 3) of an electric motor, which, through a coupling box, M, actuates an endless screw, E, which gears with a toothed wheel. This latter is carried by a drum, T, that presents upon its periphery notches whose section is represented in No. 2 of Fig. 3, and into which enter wooden disks fixed upon a cord. This cord, which is placed in two hollow columns installed in the shaft, maintains the car of the elevator. The electric motor, in running, causes the revolution of the drum, which displaces the cord and consequently the elevator. Fig. 1 gives a general view of an electric elevator of this kind.

All the safety parts that exist in hydraulic elevators are likewise found in these, and every precaution is taken to prevent accidents. The electric motor is in derivation in order to obtain a constant speed, whatever be the load. The gearings are particularly well elaborated and give a rendering of about 45 per cent.

The control of these elevators can be effected in two ways—either by an endless cord or by an electric maneuver. In the first case, an endless cord is placed in the groove of a small pulley situated above the electric motor and traverses the shaft in passing through the car. In order to set the elevator running, it suffices to pull the cord, this displacing the interrupter and closing the circuit upon the electric motor. At the story at which it is desired to stop, a button is pressed that meets with a disk upon the maneuvering cord. The latter is carried along and opens the circuit. A brake is placed upon the motor in order to permit of an instantaneous stoppage. An automatic disjunctor is also installed upon the excitation in order to break the main current, when, for any cause, the excitation is suppressed. The control of the elevator can also be effected by an electric maneuver. In the car there are interrupting buttons which act upon circuits that end at S at the upper part of the electric motor, at the rheostat R (Fig. 3), and that are connected with a small electric windlass which directly controls the starting rheostat of the elevator. No. 3 of the figure shows the connection of these wires with the interrupter. Upon pressing the starting button in the car, a small electric motor of very feeble power is actuated. At this instant, a cam properly calculated establishes a current upon this small motor for the entire time necessary for the displacement of the rheostat of the large windlass. The circuit is broken upon this small motor when the starting apparatus of the windlass has finished its travel. The duration of operation of the whole is independent of the manner in which the button is pressed; the contact once begun is continued by the cam of which we have spoken. The change of revolution is easily obtained by changing the direction of the current.

In a large number of cases it will not be easy to effect such transformations in the existing elevators. The Société d'Eclairage Electrique has devised an arrangement to which it has given the name of "hydro-electric" and that permits of the electric control of hydraulic elevators without much modification. To this effect, it utilizes a water pump actuated by a Labour electric motor. Fig. 4 gives a general view of the pump and electric motor. The pumps are conjugate and give a pressure to the water forced corresponding to a head of 40 meters. Their discharge varies, according to the diameter of the piston, between 3 and 7 liters of water per second. The general arrangements of a hydro-electric elevator are shown in Fig. 2. A reservoir, B, of a volume of about 700 liters, is placed at S in a cellar and communicates, through pipes, with a distributor, D, which is itself connected with the piston of the elevator. The electric motor, M, actuates the pump, P, which, through a pipe, F, sucks up water from the reservoir, B, and forces it under pressure to the distributor, D, and thence under the piston of the elevator. This distributor is provided with a horizontal rod that carries a lever, l, to which is attached the maneuvering rope, C. At V is seen the water conduit, which may be closed at will. The entire system is set in operation by means of an automotor, A, consisting of a weight mounted on a hydraulic piston and having a travel of about a meter. The modus operandi is as follows: Upon pulling the rope, C, the distributor, D, is opened and the automotor, A, at once descends and frees, at R, the interrupter of the rheostat, which closes the circuit upon the motor, M. The latter at once operates, and the pump, P, forces the water from the reservoir, B, under the piston of the elevator, which rises. Upon reaching the desired story, the distributor, D, is closed by the maneuver performed by the passenger, and the elevator stops. The pressure is at once re-established under the automotor, A, in consequence of the water sent by the pump. The water ascends in its place, and, in passing at R, opens the circuit. The electric motor, M, stops. When the elevator is made to descend through the maneuver of the rope in the opposite direction, the water under the piston is forced into the reservoir, B. This same water may be used over again for many times. It is a question, as may be seen, of a very ingenious arrangement that utilizes both electric energy and water under pressure.

It is now well to give an idea as to the expense of running these new models of elevators and to compare it with that occasioned by hydraulic and compressed air elevators.

The Société d'Eclairage Electrique has collected a large number of data as to the various electric elevators that it has installed. We give a summary of them in the following table:

ELECTRIC ELEVATOR.

(For one complete ascent.)

Expenditure of electric energy.	Expense in francs.
Empty, 26 watts-hour	0·01586
1 passenger, 31 watts-hour	0·0186
2 passengers, 32 watts-hour	0·0192
3 passengers, 34 watts-hour	0·0207

HYDRO-ELECTRIC ELEVATOR.

1 passenger, 52 watts-hour	0·0311
3 passengers, 75 watts-hour	0·0457

The electric elevator of which it is a question is installed upon the distribution line of the sector of Clichy Place, at Paris. It has a complete travel of 16 meters, which it effects in 43 seconds. The difference of potential at the terminals of the armature of the electric motor is 440, and 110 volts at the terminals of the inductors. The expenses are established for a complete travel up and down, and the electric energy is reckoned at the maximum price of 0·061 franc per hectowatt hour. The hydro-electric elevator is installed upon the Edison sector at 220 volts.

Let us remark that, in the figures above, the expense is sensibly proportioned to the effective load. The same is not the case with hydraulic and compressed air elevators, in which the expense remains always the same, whatever be the load.

In order to establish a strictly exact comparison between all the systems of elevators, it would be necessary to take account of the number of travels in a day and the respective loads. The Société d'Eclairage Electrique has made a comparison for a three passenger elevator in a six story building. The water is estimated at 0·6 franc per cubic meter, the output being about 275 liters per total travel.

Compressed air is sold for elevators at the rate of 3·85 francs per kilometer of travel effected. The net cost of a complete travel, up and down, is as follows:

Elevators.	Franc.
Hydraulic	0·165
Compressed air	0·077
Hydro-electric	0·045
Electric	0·025

It will be seen that the most economical elevator is the electric one, and, after it, the hydro-electric. In order to convince our readers, it would remain to give them the costs of establishment or of transformation. Such costs are essentially variable according to the installation, and it is difficult to cite figures.

We may say, nevertheless, that the cost of establishment of an electric elevator is no greater than that of a hydraulic one, and that the cost of a transformation is soon offset by the saving effected in the running. We may reckon about 10,000 francs as the cost of installing a four passenger electric elevator in a six story building, and 4,000 francs as the cost of a transformation. These figures, however, are not absolute, and may vary within wide limits.

Upon the whole, electric elevators present great economic advantages, while at the same time realizing the same conditions as other elevators. They do away with a large expenditure of city water and become during the day important centers of consumption for central stations of electric energy.—La Nature.

INVERTED ARC LAMP FOR INDIRECT LIGHTING.

WE illustrate an arc lamp, constructed by Mr. A. W. Richardson, of Skew Bridge, Worsley Road, Patricroft, near Manchester, and intended for lighting workshops by the indirect method, in which the light of the lamp is, by means of a reflector, thrown up first to a whitened ceiling, the body of the room being illuminated only by reflection from this. The system in question has hitherto found more favor on the Continent than in this country, though a very satisfactory quality of light can be obtained, there being an absence of glare, and of the strong shadows, which often prove trying to the eyes when direct lighting is used. The general appearance of the lamp as fitted up ready for use is well shown in Fig. 2, while Fig. 1 shows it with its reflector and cover

FIG. 1. FIG. 2.

ARC LAMP FOR INDIRECT ILLUMINATION.

removed, thus allowing the arrangement of the working parts to be made out. The reflector is of enameled iron, and for a 10 or 20 ampere lamp measures 7 in. in diameter at the bottom by 25 in. at the top. The feeding of the carbons is regulated by a differential brake. Each carbon holder is prolonged into a rack, and these racks gear with, but on opposite sides of, a single spurwheel. Hence, if the carbons approach or recede from each other, this wheel must rotate. The motion of this wheel is, however, controlled by a magnetic brake, which comes into action as soon as the arc is working properly, releasing the wheel again and letting the carbons approach, whenever the current between the carbons falls below its normal intensity.—Engineering.

[Continued from SUPPLEMENT, 1033, page 16508.]

ON THE ELECTROLYSIS OF GASES.*

IN the following experiments the current was produced by a large induction coil with a mercury slow break.

When the tube was filled with hydrochloric acid gas at a low pressure, the separation of the hydrogen and chlorine was seen very distinctly, the hydrogen line being much brighter on the side of the plate which acted as the cathode (which we shall call the negative side of the plate) than on the positive side, while the chlorine, on the other hand, was brighter on the positive than on the negative side of the plate.

When the tube was filled with ammonia gas, the hydrogen lines were bright on the negative side of the plate, but were absent from the positive side, while on the positive side of the plate there was the positive pole spectrum of nitrogen, and on the negative side of the plate the negative pole spectrum of nitrogen and the hydrogen spectrum.

Sulphur Monochloride.—When the tube was filled with the vapor of this substance at a low pressure, the chlorine lines were brighter on the negative side of the plate than at the positive, while the sulphur lines were brighter at the positive side than at the negative. Thus the chlorine in this substance behaves in the opposite way to the chlorine in HCl; in the latter compound the chlorine ion has a charge of negative electricity, while in the sulpbur monochloride it has a charge of positive electricity.

Influence of the Chemical Constitution of a Compound on the Sign of the Charge of Electricity on One of its Constituent Atoms.—In many organic compounds an atom of the electropositive element hydrogen can be replaced by an atom of the electro-negative element chlorine without altering the type of the compound. Thus, for example, we can replace the four hydrogen atoms in CH_4 by chlorine atoms, getting successively the compound CH_3Cl, CH_2Cl_2, $CHCl_3$, and CCl_4. It seemed of interest to investigate what was the sign of the charge of electricity on the chlorine atom in these compounds. The point is of some historical interest, as the possibility of substituting an electro-negative element in a compound for an electropositive one was one of the chief objections assigned against the electro-chemical theory of Berzelius.

When the vapor of chloroform, $CHCl_3$, was placed in the tube, it was found that both the hydrogen and the chlorine lines were bright on the negative side of the plate, while they were absent from the positive side, and that any increase in the brightness of the hydrogen lines was accompanied by an increase in the brightness of those due to chlorine. The spectrum on the positive side of the plate was that called the carbonic oxide spectrum; when first the discharge passed through the tube, the spectrum on the positive side was the so-called candle spectrum, but this very rapidly changed to the carbonic oxide spectrum. The appearance of the hydrogen and chlorine spectra at the same side of the plate was also observed in methylene chloride and in ethylene chloride. Even when all the hydrogen in CH_4 was replaced by chlorine, as in carbon tetrachloride, CCl_4, the chlorine spectra still clung to the negative side of the plate. To test the point still further, I tried the analogous compound silicon tetrachloride, inserting a small jar in the circuit to brighten the spectrum. The chlorine spectrum was again brightest at the negative side of the plate, while the silicon spectrum was brightest at the positive. This is a very favorable case for the application of this method, as there are two silicon lines (wave lengths 5058, 5043) quite close to two chlorine ones (wave lengths 5102, 5078), so that their relative brightness can easily be compared. The experiment with the silicon tetrachloride is more conclusive than those with the carbon compounds, as with the latter the spectrum on the positive side of the plate is a band spectrum, and since the potential gradient when the discharge is passing is very much steeper on the negative side of the plate than on the positive, the effects observed might be supposed to be due to the circumstances on the negative side being better adapted for the production of line spectra than those on the positive. This explanation is not, however, applicable to the case of silicon tetrachloride, where the spectra on both sides of the plate are line spectra.

From these experiments it would appear that the chlorine atoms in the chlorine derivatives of methane are charged with electricity of the same sign as the hydrogen atoms they displace.

When we can determine the signs of the electrical charges carried by the atoms in a molecule of a compound, we can ascertain whether any given chemical reaction does or does not imply interchange between the electric charges on the atoms taking part in the reaction. Thus take the reaction

$$CH_4+Cl_2=CH_3Cl+HCl.$$

If we represent the sign of the charge of electricity carried by an atom by + or − placed below the symbol representing that atom, we may write the last reaction as

$$\begin{array}{cccccccc} CH_4 & + & ClCl & = & CH_3Cl & + & HCl, \\ -+ & & +- & & -++ & & +- & \end{array}$$

so that this reaction could be produced by a rearrangement of the atoms without any alterations of their electrical charges.

If, however, we take the reaction—

$$\begin{array}{cccccc} HH & + & ClCl & = & 2HCl, \\ +- & & +- & & +- & \end{array}$$

we see that in addition to a rearrangement of the atoms there must in this case be an interchange of electric charges between the atoms; for before combination half the hydrogen atoms had a negative charge and half the chlorine atoms a positive one, whereas after combination no hydrogen atom has a negative charge and no chlorine atom a positive one. We may thus distinguish between two classes of chemical reactions (1) those which do not necessarily require any interchange of the electrical charges carried by the atom and (2) those which do. It might, perhaps, repay investigation to see whether the occurrence of chemical change is affected by the presence of a third substance in the same way in these classes of chemical combination.

Another point to be considered is the effect of this difference between the chemical actions on the amount of heat developed during chemical combination. When hydrogen and chlorine combine, the heat produced may be regarded as the joint effect of three processes:

(1) The splitting up of the molecules (H H) and (Cl Cl) into the atoms H, H, Cl, Cl.

(2) A transference of electricity by which the negative charge on one atom of hydrogen is replaced by an equal positive charge, while the positive charge on an atom of chlorine is replaced by an equal negative charge.

(3) The combination of the positively electrified hydrogen atoms with the negatively electrified chlorine ones to form hydrochloric acid.

In that class of chemical action where the atoms retain their charge (2) is absent, so that if the change in energy occurring in the process (2) were considerable compared with the changes occurring in processes (1) and (3), the thermal effects of the two types of chemical combination ought to differ considerably. If the changes in energy occurring in the process (2) had a great preponderance over those occurring in (1) and (3), the thermal effects produced by the combination of two elements ought to follow very simple laws. For if $2\{H\}$ is the excess of the energy of an atom of hydrogen charged with the negative electron over the energy of the atom charged with the positive electron, $2\{Cl\}$ the excess of the energy of an atom of chlorine charged with the positive electron over the energy of the atom charged with the negative electron, then if we could neglect the energy changes in (1) and (3) compared with those in (2), the mechanical equivalent of the heat developed when a molecule of hydrogen combines with one of chlorine to form two molecules of hydrochloric acid would be equal to $2\{H\}+2\{Cl\}$. Thus we see that if the energy changes in (2) preponderated largely over those in (1) and (3), the heat produced when an element A combined with another element B to form the compound AB, could be expressed as the sum of two numbers $\{A\}$ and $\{B\}$, where $\{A\}$ depends solely on the element A, $\{B\}$ solely on the element B. In some cases of chemical combination between dilute solutions there seems evidence that the heat produced can be expressed in this way (see Lothar Meyer, "The Evolution of the Doctrine of Affinity," Phil. Mag., vol. xxiii, p. 504), but when we attempt to apply the same law to combination between gases, it seems utterly to break down, indicating that in such cases the greater part of the changes in energy occur in the splitting up of one set of molecules and the subsequent formation of others. This view seems to be supported by the phenomena attending the discharge of electricity through rarefied gases, for the smallest difference of potential which can send a discharge through an electrified gas (which we have reason to believe involves the splitting up of molecules into atoms) is very many times the electromotive force required to liberate the ions from an electrolyte, though the latter process requires changes in the electrical charges on the ions. These reasons seem to indicate that we can hardly expect to get any clear indication of the charges carried by the atoms in gaseous compounds from the study of the thermal changes which occur when gases enter into chemical combination.

Vapors of Organic Compounds.—These show very interesting differences between the spectra on the two sides of the plate when the discharge passes through them. Thus, when the discharge first passes through the vapor of ethyl alcohol, C_2H_6O, the spectrum on the positive side of the plate is the candle spectrum, that on the negative side the carbonic oxide spectrum. For some little time after the discharge commenced I could not detect any hydrogen lines on either side of the plate; after a time, however, they appeared on the negative side but not on the positive. If the discharge was kept running for some time without letting a fresh supply of alcohol into the tube, the "candle spectrum" on the positive side of the plate was replaced by the CO spectrum, which now occurred on both sides of the plate, accompanied on the negative side by the hydrogen spectrum. This is the appearance presented by all the compounds of carbon, oxygen, and hydrogen which I examined, when the spark had been passing through them for a considerable time, and it is what would occur if the vapor were decomposed by the spark into carbonic acid, water, and hydrogen.

The appearance of the candle spectrum on the positive side of the plate with the CO on the negative was observed in many other cases. Thus on sparking through a tube filled with CO, I could not detect any difference between the spectra on the two sides of the plate, but when a little hydrogen was let into the tube the "candle spectrum" appeared on the positive side of the plate, the carbonic oxide spectrum on the negative. The same effect was observed in a tube filled with cyanogen mixed with a little hydrogen. When the tube was filled with the vapor of methyl alcohol, CH_3OH, the candle spectrum was on the positive side of the plate, the carbonic oxide and hydrogen spectra on the negative; with this vapor, unlike that of ethyl alcohol, I could not detect any stage when the hydrogen spectrum was absent.

The first explanation which occurs to one of this phenomenon is that it is owing to the potential gradient at the negative side of the plate being steeper than that on the positive, so that we may imagine we have a fierce spark on the negative side, a mild one on the positive, and that the fierce spark gives the CO spectrum, the mild one the candle spectrum. There are, however, some phenomena which seem inconsistent with this explanation; in the first place, if the current is reversed after flowing in one direction, traces of the former spectra linger for some time at the sides of the plates, and, secondly, if the difference is due to the greater decomposition at the negative side of the plate, how is it that in the case of the vapor of ethyl alcohol the hydrogen spectrum is not seen, at the commencement of the discharge, on the negative side of the plate? It only appears after the discharge has passed through for some time, when hydrogen has probably been set free by the decomposition of the vapor by the discharge. If the absence of the candle spectrum from the negative side of the plate is due to the spark being so intense that the hydrocarbon which is supposed to be the origin of this spectrum cannot exist, then we ought to see the spectra of the substances which result from the decomposition of the hydrocarbon, i. e., we ought to see the hydrogen spectrum at the negative electrode. The view which seems most in accordance with the results of observations on the discharge through these vapors is that the "candle spectrum" is the spectrum of carbon when the atom is charged with negative electricity, or of some compound of carbon in which its atom is negatively charged, while the "carbonic oxide" spectrum is the spectrum of carbon when the atom is charged with positive electricity, or of some compound in which the carbon atom is positively charged.

Discharge Through an Elementary Gas.—It has long been known that when the discharge passes through some elementary gases the spectra at the two electrodes are different. This was first shown to be the case for nitrogen, then Dr. Schuster showed that the same thing occurred with oxygen, and recently Mr. Crookes has shown that it is also true in the case of argon. I have observed a very striking change in the relative brilliancy of the red and green hydrogen lines at the two electrodes. When the tube with the plate across it was filled with hydrogen at a low pressure, then on the positive side of the plate the red line tends to be brighter than the green, while on the negative side the green line tends to be brighterthan the red; in some tubes this was so marked that on the positive side of the plate the red line was bright and the green invisible, while on the negative side of the plate the green line was bright and the red invisible. The spectroscope I was using weakened the red rays much more than the green, so that I cannot be sure that the red rays were really altogether obliterated on the negative side of the plate; the above experiment is, however, sufficient to show that on the positive side of the plate the red rays are more easily excited than the green, while on the negative side the green line is more easily excited than the red. On the negative side of the plate we have an excess of positively charged hydrogen atoms, while on the positive side of the plate there is an excess of negatively charged hydrogen atoms, and I am inclined to attribute the difference in the spectra partly at any rate to the difference in properties between a positively and a negatively charged hydrogen atom. The reason I do not attribute it wholly to the difference in the potential gradient on the two sides of the plate is that the effect is not reversed immediately, but only gradually on reversing the coil, the former spectra clinging for some time to the sides of the plate.

Chlorine.—I have made a great many experiments to see if there is any difference between the spectra given by chlorine on the two sides of the plate, but with negative results. Chlorine seems a gas in which we might expect to find this effect, for as Dr. Schuster, in his Report on Spectrum Analysis, says, the behavior of its spectrum indicates that we have several spectra superposed. I have not, however, been able to effect a separation of its spectra. The differences I observed between the spectra on the two sides of the plate were irregular, and due, I think, to impurities producing effects like those observed when the discharge passes through a compound gas. However, as has been mentioned before, there is even in the case of gases where distinct evidence of separation can be obtained, a region of pressure within which the effects are irregular, and I ascribe my failure to observe separation in the case of chlorine to my having failed to get the relation between the intensity of the discharge and the pressure so adjusted as to get outside this irregular region. The cases, however, in which distinct differences between the spectra of a single gas occur at the two electrodes seem to indicate that the spectrum given by an element is influenced by the sign of the electrical charge carried by its atoms.

I have made some experiments to determine whether there was any separation produced in a mixture of equal volumes of hydrogen and chlorine kept in the dark, when a considerable difference of potential, though not sufficient to produce discharge, was maintained between the two electrodes. The parts of the tube adjacent to the two electrodes could be shut off from each other by a tap, and the amount of chlorine in the two sides was determined by absorbing it by caustic potash. The mixture was at atmospheric pressure, and the electrodes were maintained at a potential difference of about 1,200 volts by connecting them to a large battery of small storage cells. The potential difference between the terminals was maintained for about sixteen hours on three separate occasions, but on analyzing the vessels surrounding the two electrodes, the amount of the chlorine in the vessel adjacent to the negative electrode did not differ from that in the vessel adjacent to the positive electrode by more than one per cent., and this could be accounted for by errors of experiments, as test experiments, in which the mixture had not been exposed to the electric field, gave differences comparable with these. We should conclude from the preceding experiments that the molecules of a gas are not acted on by any appreciable translational force tending to move them from one place to another, when they are near to a body charged with electricity. To test this point further, two large terminals were placed in bulbs which were connected by a horizontal capillary tube, in which a drop of sulphuric acid was placed; a difference in the pressure of the gas would cause the sul-

* Paper read at the Royal Society, by J. J. Thomson, M.A., F.R.S., Cavendish Professor of Experimental Physics, Cambridge.—Nature.

phuric acid to move, and the arrangement acts as a very delicate pressure gage. The bulbs and tube were filled with chlorine at atmospheric pressure. The terminals were then connected to the electrodes of a battery giving a potential difference of 1,300 volts, but not the slightest movement of the drop of acid could be detected.

I wish to acknowledge the help I have received in making the preceding experiment from my assistant, Mr. E. Everett.

PUBLIC HYDRAULIC POWER SUPPLY.*

NOTES ON HYDRAULIC POWER SUPPLY IN TOWNS, GLASGOW, MANCHESTER, BUENOS AYRES, ETC.

By Mr. EDWARD B. ELLINGTON, of London.

THE distribution of hydraulic power in towns has not previously been brought directly before the members of this institution; and the fact that works of this character have just been completed in Glasgow renders the present meeting a suitable opportunity for dealing with an engineering subject of such continually increasing importance.

Existing Hydraulic Power Works.—The present position of the various undertakings which have been established for this purpose is summarized in the following Table I.

TABLE I.—*Existing Hydraulic Power Works.*

Place.	Year of Establishment.	Length of Mains.	Largest Diameter of Mains.	Horse-Power.	Delivery of Water per Week.	Number of Machines Worked.	Pressure per Square Inch in Mains.
		miles	in.	I.H.P.	gals	No.	lb.
Hull ..	1877	9¼	6	250	100,000 to 500,000	58	700
London .	1884	76	7	3460	9,500,000	2300	780
Liverpool	1888	18	6	800	1,000,000	452	800
Melbourne ..	1889	18	6	800	1,500,000	413	780
Birmingham	1891	3¼	6	52	78,000	..	700
Sydney ..	1891	12	6	688	740,000	250	750
Antwerp	1894	44	12	1000	3,200,000	Turbines at three stations	750
Manchester ..	1894	12	6	800	1,000,000	247	1120
Glasgow..	1895	9	7	600	nil	nil	1120

General Arrangement.—Of the hydraulic power works in London full particulars have already been given to the Institution of Civil Engineers; and as all such works have much in common, it is hardly necessary to add any detailed account of the most recent establishments in Glasgow and Manchester. The supplies in these towns differ from those given elsewhere in the higher pressure employed, namely 1,120 lb. per square inch. This pressure was adopted in Manchester on the recommendation of the author and his colleague, Mr. Corbet Woodall, in order to render the power available under the most economical conditions for working the numerous hydraulic packing presses within the area of supply in that city. The works in Manchester have throughout been carried out under the supervision of Mr. Woodall, acting for the corporation. In Glasgow also there is a considerable demand for power for pressing; and Mr. Gale, who is here responsible for the general scheme, considered it advisable to be able to supply the same higher pressure. The plans and specifications for these works were prepared by the author's firm as consulting engineers; and their execution has been under the control of the water department of the corporation, and especially of Mr. Cochrane, Mr. Gale's assistant mechanical engineer. The general arrangement of the works in Glasgow and Manchester is shown in the diagrams, Figs. 1 to 3. The engine house in each city is laid out to contain six sets of vertical triple expansion engines of 200 indicated horse power each, of the kind introduced by the author in 1880; but in the earlier engines the cylinders were arranged compound, with one high pressure and two low pressure cylinders. The scale on which the two stations have been planned is the result of the experience gained in London, which has demonstrated that for hydraulic power supply a station of about 1,200 horse power is the most economical both in working expenses and in outlay of capital.

Engines.—The principal dimensions of the pumping engines in Glasgow (Fig. 4) are as follows: High pressure cylinder, 15 in. in diameter; intermediate, 22 in.; low pressure, 36 in.; and all three 24 in. stroke; cranks set at 120°, in the sequence—high, low, intermediate; pumps, single acting, 13 in. in diameter and 16 in. stroke; surface condenser 580 square feet of condensing surface; crankshaft journals, 7 in. in diameter; flywheel, 7 ft. in diameter, weighing 2 tons. The engines are designed to work with a steam pressure of 150 lb. per square inch above atmosphere, and this is the boiler pressure adopted in the recent stations in London and Glasgow; but in Manchester the pressure of only 120 lb. is used, at the request of the committee of the corporation who had charge of the undertaking.

The Glasgow engines have not yet been tested for economy, but the London station of similar character at Wapping gave a consumption of 14·1 lb. of water per indicated horse power per hour. (Proceedings, 1894, pages 536-551.)

Boilers.—The boilers in Manchester are of the usual Lancashire pattern, five in number, 7 ft. 6 in. in diameter and 30 ft. long. In Glasgow, as coke breeze is to be used for fuel, and as there is ample space under the reserve water tanks, it was determined to arrange the house for eight Lancashire boilers, 7 ft. in diameter and 30 ft. long; at present only four have been fitted for the three sets of engines in place. Since the station was designed, the corporation have determined to add to the boilers Watmun automatic circulating apparatus for increasing their efficiency. Both in Manchester and in Glasgow two of Green's economizers, with 160 tubes each, have been fitted at the back of the boilers (Fig. 4).

In the recent Wapping and City Road stations in London, Fairbairn-Beeley boilers have been used, as shown in Figs. 5 to 7; and these, with the economizers, have given under trial the remarkable result of 11·11 lb. of water evaporated from the temperature 63·5° of the hot well feed, and at the temperature of 352° corresponding with 143·3 lb. pressure above atmosphere, or 13·44 lb. of water evaporated from and at 212°, with 1 lb. of dry Nixon's coal, having a calorific value of 15·5 lb.; the thermal efficiency of the boiler was therefore 86·7 per cent. It will be interesting to compare this result with that of the Glasgow boilers when they can be tried.

Accumulators.—The accumulators at the Manchester and Glasgow stations are of the same capacity, Fig. 4, namely, two at each station, having rams 18 in. in diameter and 23 ft. stroke, and each loaded with 127 tons, inclusive of ram and casing.

Delivery.—Each set of engines is designed to deliver 230 gallons per minute at a speed of 60 revolutions or 240 ft. of piston; but it is not intended that they should be run in regular work at more than 50 revolutions or 200 ft. per minute. Assuming that five engines are working at the same time, the total regular working capacity of the stations when fully equipped is 57,500 gallons per hour at 1,120 lb. per square inch.

The maximum recorded during regular work in an hour at the Wapping station of the London Hydraulic Power Company, with five engines running, has been 66,000 gallons at 800 lb. per square inch, or an average for each of the five engines of 45 revolutions per minute. The reason for dividing the power at a station into so many units is, of course, that the demand for power, as for lighting, is so irregular, as shown in Fig 8, that the maximum demand is maintained approximately for only an hour or two during any day.

Site of Station.—The general arrangement of the Manchester and Glasgow stations has been determined mainly by the configuration and situation of the site selected. At first view the proposed site for the Glasgow works appeared difficult to lay out satisfactorily; but the solution ultimately arrived at, the author thinks, has fulfilled the required conditions in a fairly satisfactory manner, both mechanically and architecturally. The problem was solved by dividing the site into two areas at different levels by a heavy retaining wall, Fig. 9; and an attempt has been made to give the buildings a more imposing character than has been usual in such works. The position of the site seemed to require this, and the suggested elevation was at once accepted by the committee as being appropriate to its surroundings.

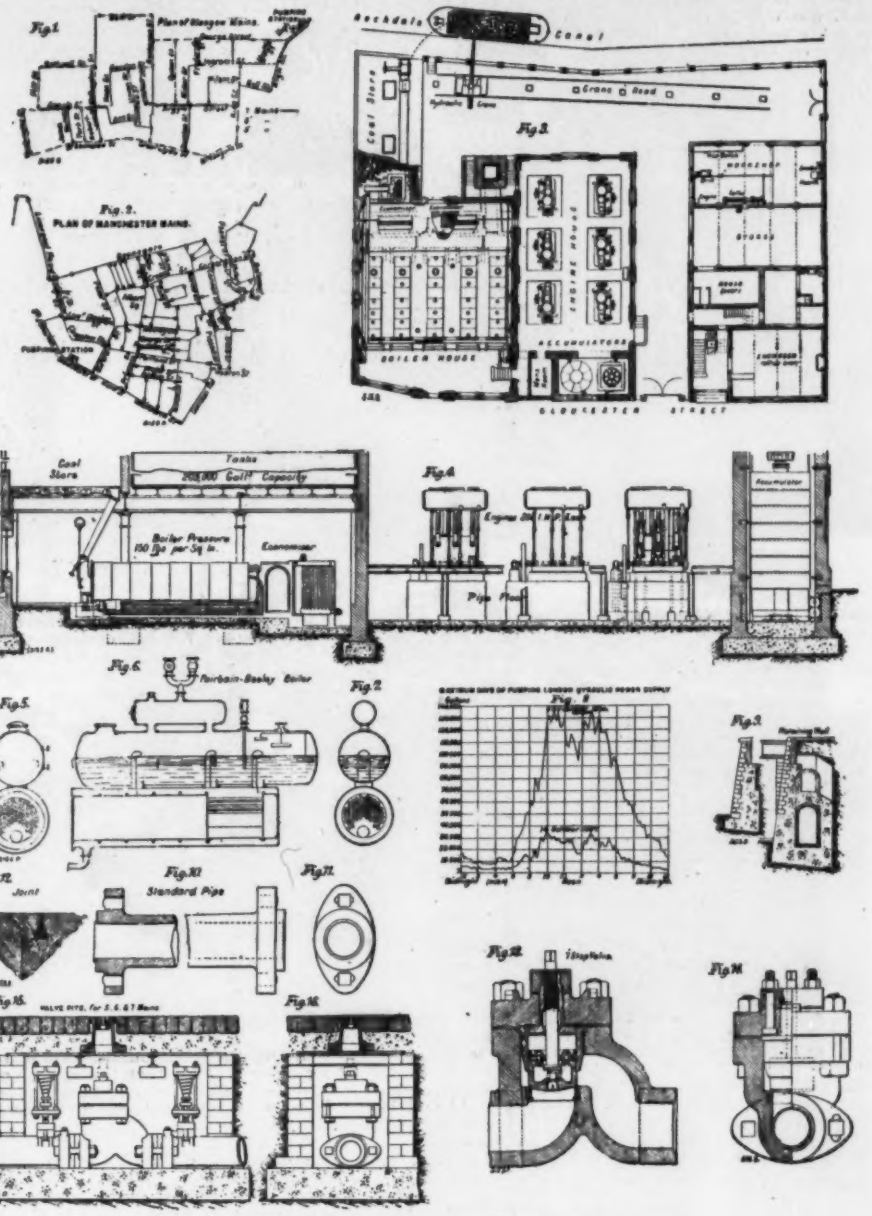
Water.—Both in Manchester and in Glasgow the water supply is taken from the corporation mains; so that no pumping is required to fill the tanks, and no filtering apparatus is needed. The whole arrangement of these stations is thereby much simplified as compared with those in London, where Thames or canal water is used; it is also unnecessary to have the same large reserve of water in the tanks. At the Wapping station, in London, for instance, the tanks over the boilers have a total capacity of 237,000 gallons; and, in addition, there are underground reservoirs of filtered water, containing 500,000 gallons. In Manchester and in Glasgow the whole storage provided is only 230,000 gallons and 200,000 gallons respectively.

The pipes in the engine and boiler houses are all arranged on the duplicate or circuit system; and great attention has been given to the provision of sufficient access to all parts of the machinery.

Mains.—The supply from the stations in Manchester and Glasgow is taken to the streets through four mains, which are laid in circuit, with valves at intervals of about 440 yards. Connecting mains have a valve at each end, so that the majority of buildings supplied have two ways by which the power can be obtained. The largest mains laid in Manchester are 6 in. in internal diameter, and in Glasgow 7 in., Figs. 10 and 11. The reason for the adoption of 7 in. mains in Glasgow is that there is a tendency to reduction of bore through corrosion due to the action of the Glasgow water, in consequence of which Mr. Gale wished the mains to be larger than the usual size. The mains at present laid, or in progress of laying, in Manchester and Glasgow are shown in Figs. 1 and 2. Sections of the mains, stop valves and valve pits are shown in Figs. 10, 11, 13, 14, 15 and 16.

Joints.—The joints of the pipes are precisely the same as those used for the lower pressures in London and elsewhere, as shown in Fig. 12. The packings are gutta percha rings inserted in a V-groove, and the pipe ends are spigot and faucet. The setting back of the flanges, so as to leave a considerable projection of the pipe ends beyond them, has greatly strengthened the pipes, without adding any weight; and so far as is known, only one failure of pipe flange made on the author's plan has occurred during the last ten years. This failure occurred through a settlement in Bishopsgate Street, London. With pipes made without the flanges set back, many failures have occurred.

Registration of Supply.—Always supposing that the demand for the power is large enough, the commercial success of public hydraulic power stations is largely dependent upon the proportion of the total output which can be accounted for by the meters employed. It is impossible to obtain a high efficiency of the machinery in use without a great deal of time and attention being given to the registration of the supply. The means employed for registration, therefore, and for the maintenance of efficiency, are of great interest and importance in connection with the distribution of hydraulic power. This particular question has not been



HYDRAULIC POWER SUPPLY.

* Paper read before the Institution of Mechanical Engineers.—Engineering.

previously dealt with by the author so fully as to render it superfluous to revert to it here. The London supply of power is so largely in excess of anything required elsewhere, that the experience gained in London is the most valuable; and the present remarks on this head will apply mainly to London.

Efficiency.—The first two matters to determine are the quantity of power delivered by the engines into the mains, and the rate of flow as far as possible throughout the day. The quantity of power is ascertained by a record of the engine revolutions obtained with the usual counters. A constant is used, which for the London engines is reckoned as 4·8 gallons per revolution. This constant has been verified from time to time by observing the quantity delivered from the tanks during some hours by direct measurement; and no alteration is allowed in this constant under any circumstances. The counter readings are taken in London at 6 o'clock in the morning and 6 o'clock at night, and are entered in the engine room log book. These readings give the amounts from each station every 12 hours: and the returns from the several stations are collected and entered in a general log book. Every 6 or 7 weeks the meters are read on the consumers' premises, and the totals are collected and the efficiency of the system is determined by the fraction representing the ratio of the quantity registered by the meters to the quantity pumped as determined by the counters. This ratio in London is found to vary considerably in different quarters of the year, and in different years. It has never been higher in any quarter than 0·9826, and has never been lower than 0·8560. The following is generally the case, the exhaust water only is registered; and to drainage from consumers' machines and pipes. The last loss is considerable in winter, when consumers make a practice of draining their pipes after work every day.

(To be continued.)

ITALIAN STEAM WINCH FOR HAULING DOWN WAR BALLOONS.

The use of balloons for purposes of military observation now forms a regular adjunct to the equipment of all first class armies. The hydrogen gas necessary for the inflation of the balloon is carried to the field in steel tubes, in which the gas is greatly compressed. The tubes are then connected together and joined to the balloon, which becomes quickly inflated. In the Italian army they make use of a steam winch for quickly hauling down the balloon, and the machine then travels, taking with it the balloon, to another point of observation, when it is allowed to ascend. The military officer who ascends in the balloon takes photographs of the country below him, makes sketches and general observations useful for military purposes. A telephone line extends from the balloon to the ground. We are indebted to L'Illustrazione Italiana for our engraving.

THE RETURN OF LIEUTENANT PEARY.

After a series of thrilling adventures, R. E. Peary, C. E., U. S. N., and his comrades have returned in safety. They were taken on board the relief steamer from Peary's headquarters at Anniversary Lodge, which the Kite reached on August 3. The steamer started to return on September 1. On reaching St. John's, N. F., Lieutenant Peary traveled immediately to Portland, Me.

In brief, the story of the expedition is as follows: He reached Independence Bay on the northeast coast, and, after making heroic efforts to continue the work, was defeated by the crippled state of his party and equipment. Some of the supply caches on the ice cap were buried from fifteen to twenty feet under the snow where they could not be found; then came the days when Peary and his companions lived on frozen walrus meat which was intended for the dogs, and musk ox meat when it could be obtained. The men and dogs were enervated at an altitude of seven thousand to eight thousand feet above the level of the sea. The sledges and foot gear were ruined among the rocks of this northern land, and at last it was seen that to make further progress would mean death, so the homeward march was begun, which occupied twenty-five days. When Anniversary Lodge was reached, on June 25, only one dog survived out of the sixty-three that started on April 1. The last of the provisions were consumed when twenty-one miles from Anniversary Lodge. Last year Peary failed and was forced to return on account of stress of weather, as noted in the Scientific American for September 23, 1894. This year he failed again on account of the loss of his food supply and insufficient support, two such brave men as Hugh Lee and Matthew Hensen not being sufficient to adequately support the explorer.

After devoting years to Arctic study and four years of actual labor in the Arctic regions, Mr. Peary has again been foiled, through no fault of his own, and though he has won brilliant success, he has also met with the disappointment which has been too often the portion of Arctic explorers. It is, however, very doubtful if the net results of any Arctic voyages have been more fruitful than those of the five Peary expeditions. It is too early as yet to sum up accurately these results, but as Mr. Peary announces that he will make no more trips to the north, it is now expected that these researches will be published in book form. His studies among the glaciers, the increased knowledge of the Inglefield Gulf region, the record of annual meteorological observations and descriptions of the Arctic highlanders are considered by Dr. Fred. A. Cook, who accompanied Mr. Peary's expedition of 1891–1892, as of really more importance than the discovery of the North Pole. The Kite returned with the most valuable collection ever brought from the Arctic region. It is mainly the work of Professor Dyche, of Kansas University, who collected nearly four thousand specimens of birds, eggs and animals in the neighborhood of Holsteinburg. In addition there were secured during the Kite's cruise twenty-four walruses, three narwhals, twenty-five seals, thirteen polar bears and a number of other animals. Several large and important meteorites were also obtained, one weighing four thousand pounds. The collection made by Professor Dyche will be divided among the American Museum of Natural History and the Kansas State University.

When asked whether he thought the North Pole would be reached, Mr. Peary said: "I believe it will,

ITALIAN STEAM WINCH FOR HAULING MILITARY BALLOONS.

are the ratios of efficiency for the 10 years 1885 to 1894 in London and for 6 years in Liverpool:

	London.	Liverpool.
1885	0·8665	....
1886	0·9250	....
1887	0·9400	....
1888	0·9370	....
1889	0·9545	0·9450
1890	0·9576	0·9590
1891	0·9089	0·9853
1892	0·9240	0·9304
1893	0·9119	0·9543
1894	0·9181	0·9592
Average	0·9243	0·9555

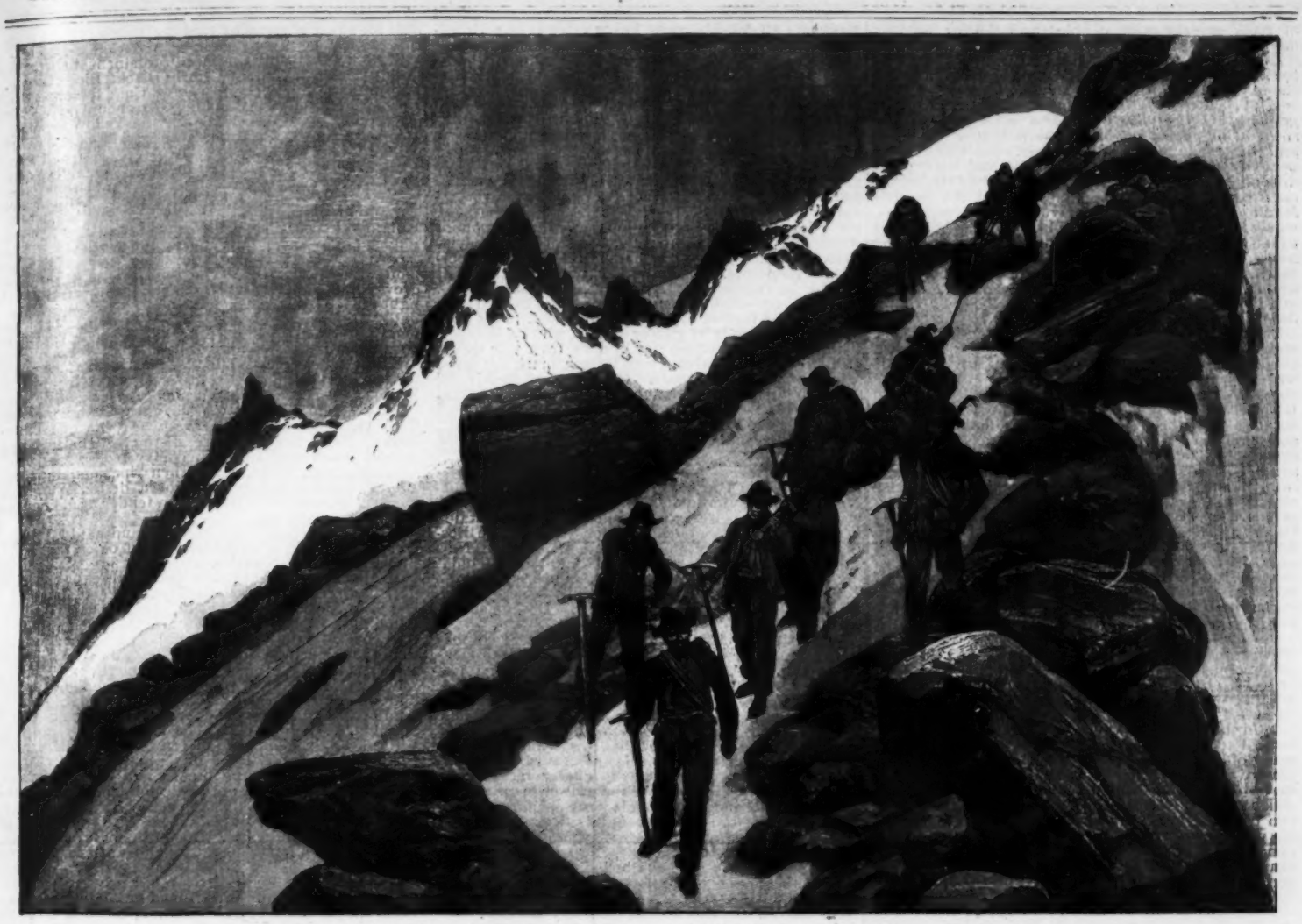
The causes which produce the variations in these ratios of efficiency are themselves very variable; and with an extensive supply, such as obtains in London, it would be almost impossible to identify the locality of the losses if the record of daily deliveries and the quarterly reading of meters were all there was to depend on. The sources of waste and error are of two kinds; first, those which occur before the supply reaches the consumers; and secondly, those which appertain to the consumers' machinery and to the meters. The losses of the first kind are made up of leakage past the suction valves of the pumps; of leakage in the mains, and in the packings of the valve glands connected with the mains; and of losses in frosty weather by pumping water to waste for the purpose of maintaining the circulation. The losses of the second kind are due to defects in the registration by the meters; to leakage from consumers' machines, where, as

and in a comparatively short time. I am, however, no believer in the existence of an open polar sea."

EMILE REY'S LAST ALPINE JOURNEY.

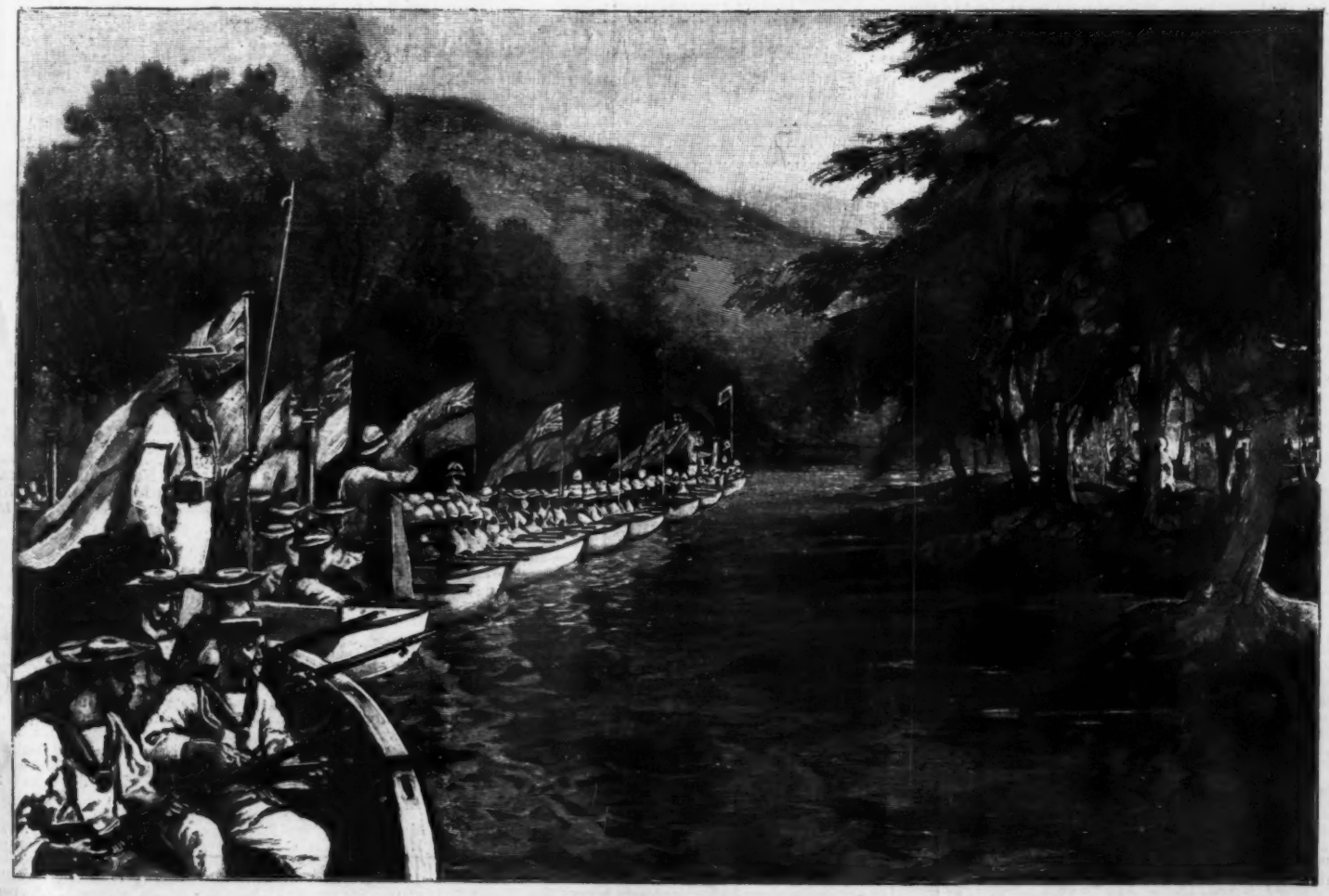
A pathetic interest attaches to our illustration of the last journey across his beloved Alps of Emile Rey, who lost his life the other day descending the Aiguille du Géant. The famous guide had a very large circle of English friends, with whom he had made many mountaineering expeditions. He was a native of Courmayeur, where he was born in 1846. He very soon began to climb the various peaks surrounding this renowned village, and in 1876 Lord Wentworth retained him for the Alpine season, an engagement renewed for the two following years. One of Rey's noteworthy accomplishments was when he made the third, fourth, and fifth ascent of the higher peaks of the Dru in four consecutive days. It was then that the peak was climbed for the first time from Montanvert without sleeping in the hut. Rey crossed the summit of Mont Blanc amid the deep snows of January, 1888, from the Aiguilles Grises to Chamounix. The famous guide used often to mention with pleasure his visit in 1864 to this country, and his ascent of Ben Nevis. He was always very modest as to his own achievements, and was conscientious and trustworthy, like most of the Alpine guides. His death is felt as a personal loss by the many climbers who have had his company on the white peaks where he was so much at home.

The facts, as far as they have been carefully collected by Mr. C. E. Matheus, a former president of the Alpine



EMILE REY, THE ALPINE GUIDE—RECOVERY OF HIS REMAINS.

Club, are to the following effect: On August 23, Mr. A. C. Roberts, a Preston gentleman, engaged Emile Rey to climb with him what is popularly known as "the little Dru." They slept at the Couvercle, climbing on the following morning the Aiguille du Géant. Thence they began to descend at 3:20, amid the threatenings of a storm. They came to a steep gully, involving a descent of 600 ft., and here, fearing no danger, Mr. Roberts and his guide unroped. Rey was leading. Mr. Roberts was standing at the top of a small chimney down which Rey had climbed. Rey jumped or allowed himself to drop into a small plateau



DRAWN BY J. NASH, R. I. FROM SKETCHES BY CAPT. F. S. DUSMORE, R.N.R.

While the flotilla was being towed by the steam launches up the Kilifi River some of Mbaraka's scouts were observed strolling along under the mangroves on the river banks, and a signal was made from the commodore's launch, "Be ready to fire."—The Graphic, London.

THE GUNJORA EXPEDITION IN BRITISH EAST AFRICA.

of rock, covered with pebbles and sloping slightly downward.

His foothold failed, and he fell with three bounds on to the glacier below, not by the ordinary route, but on the other side of the rocky spur by which the descent is made. Mr. Roberts attempted to reach the ill-fated guide, at first by the rocks and afterward by the snow below them, but dense snow clouds rolled up, and the attempt proved hopeless. He shouted repeatedly, but there was no answer, and with great difficulty and in a raging storm he reached the refuge on the Col du Géant alone. In the hut Mr. Roberts found a party of six Swiss travelers, with six guides, who had, with considerable difficulty, made their way to the summit through the storm. The next morning he begged the assistance of some of these guides, but this assistance was refused. News of the disaster was, however, carried by them to Courmayeur, and a party of guides ascended and recovered the body at 7 A. M. on August 26.

Yet another fatal accident has happened in the Alps, and this time the victim is a lady, Miss Sampson, a climber of no mean repute. "I am sorry to see," writes a correspondent, "that Peter Anton Biner was one of the guides on the ill fated expedition to the Rothhorn, for he proudly boasted to me when I was in Zermatt early this season that he had never had an accident happen to any one in his care. If courage, experience, and great personal strength could have availed, there is no guide on whom one might have more surely relied, but in this case the bête noire of mountaineers—falling stones—was the cause of the catastrophe. Miss Sampson, Miss Growse, a guide named Carrel, and Biner, left the Riffelalp at two A. M., on August 30, for the Trift Pass. All went well till shortly after ten o'clock, when just as they issued from the pass to ascend the glacier a perfect cannonade of stones rattled down upon them. Carrel managed to leap aside and escape the avalanche, but Miss Growse was knocked down, and the knapsack on Biner's back was flattened by the stones. A large bowlder fell on Miss Sampson, and she dropped to the snow without a groan, and a few minutes later, when the avalanche had swept by, they found her spine had been broken. She did not recover consciousness during the forty minutes for which she lingered, and died quite peacefully in the presence of her friend and the guides. The funeral took place in Zermatt a few days later, and many of the visitors followed the sorrowful little cortège to its last resting place." Thus, within a short space of time, there have been added to the fatality which befell Mr. Benjamin Eyre, earlier in the season, two other accidents on the Alps. The fine weather has perhaps induced mountaineers to be more venturesome than usual, but "the unexpected" is really the cause of most of these sad disasters.—Illustrated London News.

[Continued from SUPPLEMENT, No. 1033, page 16516.]

CHERRIES.*

By L. H. BAILEY and G. H. POWELL.

FERTILIZERS.—The cherry probably requires as little fertilizer as any fruit grown. An occasional crop of crimson clover turned in will generally furnish sufficient nitrogen and improve the soil in other ways. Potash can be furnished in wood ashes or in a high grade muriate of potash, using 250 pounds per acre of a 50 per cent. muriate. This should be applied in the spring and harrowed in. Phosphoric acid may be applied in the same proportions in the form of bone compounds or in South Carolina or Florida rock. In good soil, it is seldom that the cherry orchard needs heavy fertilizing if clean culture is practiced; the close observer can tell when to apply plant food by the action of the trees themselves.

Limits to the Profitable Age.—The cherry will live to a great age and bear fruit, there being records of such trees over a hundred years old. As the cherry industry is so small, and no great number of trees have been treated as an orchard for a long time, it is difficult to say just how long an orchard will continue to be profitable. This will depend largely on the variety. In general, I should say that thirty years is the limit to the most profitable age. After that time the trees become so large that the expense of picking the fruit and caring for the trees increases rapidly.

Handling the Crop.—Before one goes into the sweet cherry industry as a business, it should be clearly understood that the cherry is a delicate fruit and more susceptible to injury from handling and from changes in the weather than the strawberry, and the industry should not be taken up unless plenty of good pickers can be obtained on short notice and unless desirable markets are within reach in eight to ten hours after the fruit is picked.

It is one thing to raise a crop of fruit, but an entirely different thing to handle and market it successfully. These remarks apply with particular emphasis to the sweet cherry, because the crop has to be sold immediately when ripe and the delay of a day may mean the loss of the entire crop, as the commission men "slaughter" the sales when the fruit begins to go down. It is strongly recommended that the markets be thoroughly looked up and studied before one goes into the cherry business.

The first essential in handling a crop of cherries is to have the fruit picked with great care, the stem being left on each cherry, and only the stem touched with the fingers. The most desirable method of picking is in eight pound baskets, as in a larger package the bottom fruits are pressed too heavily. I saw delicate Tartarian and Governor Wood picked in half bushel baskets last summer and then turned into twelve pound baskets for shipping, and the grower wondered why his cherries got into market, which was only three hours away, in such poor condition! The handling of cherries and walnuts should never be confused! Unless pickers are closely watched, a good many fruit spurs will be broken off, especially if the fruit has a tendency to grow in clusters.

This should be carefully watched, as it destroys the fruit buds for the coming year. The fruit should be picked a few days before ripe. Pickers earn $1.50 to $2 per day in a good crop, at one cent per pound.

The manner in which fruit is placed on the market, especially all delicate fruits, has as much to do with selling it as the quality of fruit itself. The demands of the market should always determine the method of packing. This can be learned by correspondence with reliable commission men, who would often obtain better prices for cherries and other fruit, if their advice were asked and followed. For the general market, there is probably no better package for the cherry than the eight-pound climax grape basket, but for the finer classes of cherries and the retail trade (which should always be worked up for the finest cherries) a smaller package is more desirable. There are several packages which hold from six to twenty boxes or baskets, the whole package weighing not more than 40 pounds when full, which are desirable. A package heavier than 40 pounds will be roughly handled by transportation companies. In the small packages the fruit should be made very attractive. All stemless or bruised cherries should be thrown out, and the top layer of fruit faced in rows with the stems hidden. This work can be done rapidly by girls or women, who lay the cherries on the bottom of the box in rows, fruit side down, then fill the box, nail and turn it over, mark the faced side and put it in the crate. If baskets are used in the package instead of boxes, the top of each basket should be faced. The extra cost of facing the fruit pays in the ready sale which it brings.

I will recite a bit of our own experience of the present year:

Black Tartarian and Napoleon Bigarreau cherries were packed in two styles of packages, the fruit being handsomely faced in both cases. A spring crate or case holding six boxes of cherries each containing 6 pounds (36 pounds), sold for $1.50 by one Boston firm and $1.75 by another, and $1.75 by a New York firm, or 4½ cents per pound. Another case holding twenty two-pound baskets (40 pounds), sold by the Boston firm for $3.75 and $4.00, and by the New York firm for $4.00, or 9½ cents per pound. Both packages cost the same.

In another instance, Black Tartarians were shipped in five-pound baskets and sold for 30 cents, or 6 cents per pound, while those shipped in eight pound baskets brought 65 cents and 70 cents, or 8½ cents per pound.

All these remarks apply to fruit sold in the general market; but there is a great demand for sweet cherries from the canning factories, for which the fruit, while carefully harvested, is less laboriously packed.

Profits.—The profits from the cherry industry depend mainly on the efforts of the grower in producing first-class fruit and in placing it on the market somewhat after the directions given in the previous topic. In 1888, an acre of cherry trees, 18 years old, including Black Tartarian, Black Eagle, Napoleon Bigarreau, Elton, Yellow Spanish and Downer's Late Red, netted $380, while an acre of rye netted $8.

The following sample figures are taken from sales from the orchard this season, trees 25 years old:

Five trees of Robert's Red Heart averaged 280 pounds per tree; the fruit sold for 9 cents per pound, bringing		$25.20
The expenses were:		
Picking	$2.80	
Packages	1.40	
Packing	1.25	
Express	2.80	
Commission	2.52	10.77
Net profit per tree		$14.43

One tree of Robert's Red Heart yielded 416 lb., which sold to retail dealers at 10 cts. per lb. at the express office,		$41.60
Expenses:		
Picking	$4.16	
Packages	1.56	
Picking	1.75	7.47
Net profit		$34 13

One acre of Windsors containing 70 trees 8 years old, yielded 84 lb. per tree—5,880 lb., which sold at 10 cts. per lb.,		$588.00
Expenses:		
Picking	$58.80	
Assorting and packing	20.00	
Packages	30.00	
Express and commission	70.00	
Cultivation (plowed once, and harrowed six times)	3.50	
Fertilizers (300 lb. potash, 100 lb. bone, 15 lb. crimson clover seed)	4.25	
Interest on land at $1.50 per acre	9.00	195.55
Net profit		$382.45

All these figures refer to sales in the open market. There is a good demand for sweet cherries for canning factories. The canners generally prefer the "white cherries"—those with a white juice and rather light-colored skin. The crop of sweet canning cherries in western New York appears to be growing smaller, and the California product has driven out much of the home made goods. One of the best informed canners in the western part of the State writes as follows concerning the sweet cherry pack: "Up to six or seven years ago, we handled from fifty to one hundred tons annually. The cherry crop appears to be growing smaller each year, and to be deteriorating very much in quality. Our output on cherries used to be composed largely of the white varieties, and we used to put up fine grades that were esteemed very highly in New York and the Eastern markets. Some years ago, California began to can cherries, and subsequently put them on the Eastern markets in competition with the finer grades of Eastern cherries. The result was that the California product drove the Eastern canned cherries almost entirely out of the market, except in some of the cheaper grades. The California cherry is much finer in appearance, is larger, freer from worms and imperfections, and also possesses a very fine flavor."

The canners tell us, in general, that when they can get good fruit, they have no trouble in making a salable product. It is evident that good fruit cannot be obtained, year by year, unless the trees are planted in such way that they can be well cared for. The price paid for sweet cherries by canning factories runs from three to five cents per pound.

Varieties.—Among the most prominent hearts are Belle d'Orleans, Knight's Early Black, Black Eagle, Black Tartarian, Kirtland's Mary, Coe's Transparent, Downer's Late Red, Elton and Governor Wood. The most prominent Bigarreaus are the Yellow Spanish, Mezel, Napoleon, Rockport, Tradescant's Black Heart (Elkhorn) and Windsor.

For general market purposes, the firm fleshed varieties of a black or red color are preferable, as they ship better, do not show finger marks from handling and are not so susceptible to the rot. From the Hearts, Knight's Early Black, the Black Tartarian, Black Eagle and Downer's Late Red might be added.

Governor Wood is probably the cherry most common to western New York. The tree is a vigorous grower and forms a regular, round head. The fruit is light red or light yellow with a red cheek, short cordate, soft, sweet and good. Peduncle of medium length, inserted in a broad cavity; flesh nearly colorless. This variety is an excellent one for home use, but cannot be recommended for market, because of its tender, light flesh, and its great susceptibility to rot. Ripens about the middle of June.

Elton.—The Elton is another cherry commonly set. Tree vigorous, upright, leaves with darkened footstalks. Fruit heart pointed, distinct in shape, large, yellow, much overlaid with cherry red. Flesh firm, becoming soft, white, juicy when ripe, and luscious. Heavy, regular bearer, but not a desirable market variety, because of its light color, tender flesh and susceptibility to the rot.

Black Tartarian.—Tree vigorous and rapid grower, erect when young, spreading when older, the large limbs losing side branches giving the lower interior a bare appearance. Fruit attached by threes, short cordate, not pointed. Flesh dark purple, soft, but firmish; deep, dark red or black. Juice very sweet and abundant. Stone small. Peduncle 1½ in. long, set in a flattened, shallow cavity. Regular and heavy bearer, quality excellent. Ripe the middle of June. The Tartarian is the best black heart for market and family purposes. It does not rot as badly as the light hearts, and though not as firm as desirable, its high quality, regularity in bearing, and dark color recommend it strongly.

Black Eagle.—Tree a rapid grower, erect with roundish head, top dense, large limbs not bare as in Tartarian. Fruit large, borne in pairs and threes, in thick clusters on the spurs, obtuse or pointed. Color same as Tartarian and slightly more acid. Flesh same color. Quality excellent. Moderate bearer. Does not rot as badly as the light hearts. One of the best dark hearts for market and family use. Ripe just after the Tartarian.

Downer's Late Red.—Free, rapid grower, head upright and roundish. Fruit medium size, roundish, heart shape. Skin of a delicate red, mottled with amber where shaded, very tender, melting, luscious. Fruit hangs for considerable time on tree. A heavy, regular bearer. Does not rot badly. Ripe about July 8. A good, late, tender variety.

There are many other heart cherries that might be described, but it is the intention of this paper to give only the leading varieties; some may have been omitted and their descriptions can be found in the leading horticultural books. The same remarks may also be applied to the firm-fleshed kinds, only the leading varieties of which will be given.

Napoleon.—Tree medium size, erect with roundish head. Fruit borne generally in twos; very large, oblong cordate; light lemon yellow with red cheek in the sun. Flesh very hard, brittle, colorless, reddish at stone. Stem medium length, stout, in a moderately deep, even cavity. Good. Excellent bearer. Ripe about June 20. Rots badly when ripe and splits if left too long. The Napoleon Bigarreau is probably the most desirable light colored cherry for market purposes. Its hard flesh and large size make it a good shipper and an attractive fruit when placed in small packages. Although it rots badly, if picked as soon as well colored and before ripe, this difficulty will be largely obviated. It must be watched closely in humid weather and when the first signs of rotting appear, the crop must be picked or it will be lost.

Robert's Red Heart.—The following description applies to a variety of that name grown in eastern New York. The description given in Downing is not clear enough to positively establish its identity, but the history of the plantings seems to establish its name beyond a doubt. Tree erect, not spreading, roundish, vigorous grower, dense. Fruit short cordate, as large and as fine as the Napoleon, in large clusters; bright dark red, with an under mottling. Peduncle long, set in a moderately deep, broad depression. Flesh pinkish; subacid. Juice nearly colorless. Handsome. Quality fair. Does not rot as badly as the Napoleon. Very heavy regular bearer. Ripens with the Napoleon. The fruit should be faced in fancy packages. One of the best firm-fleshed cherries.

Mezel (Bigarreau de Mezel).—Tree a vigorous upright grower; leaves large. Fruit very large, obtuse, heart shape, flattened on both sides, uneven. Skin dark red to black. Firm, but heart like, juicy, meaty, very sweet and rich. Stem long, slender and tortuous. Handsome and excellent. Said to be a prolific bearer, though I have seen only a few trees in fruit. Ripe about June 20.

Windsor.—Tree upright, vigorous and rapid grower, leaves large. Fruit large, roundish oblong, very firm, juicy, mottled red; flesh pinkish; sometimes streaked. Peduncle medium length, stout, set in a slight broad depression. Quality good. Heavy bearer. Ripe about July 4. Attacked freely by curculio. The most desirable late cherry either in the firm or tender fleshed varieties. It hangs a long time and does not rot badly.

The Dukes are chiefly represented in New York by May Duke, Reine Hortense, and Belle de Choisy. The May Duke is a large very dark red cherry, short cordate, soft, the flesh colored and subacid, of excellent quality. One of the best family cherries, but little grown for market because of its softness. A peculiarity of the May Duke is its habit of ripening unevenly. It is not infrequent that one branch or one part of the tree matures its fruit while the remainder of the crop is still green. Sometimes the two sides of the same fruit mature at different times. Reine Hor-

* Abstracts from Bulletin 98, Cornell University Agricultural Experiment Station, Horticultural Division.

tense is an oblong cordate, light yellow cherry overlaid with amber and splashes of light red. Flesh nearly white, very soft, subacid, of medium to good quality. Rots badly. Belle de Choisy is one of the best of dessert cherries, but is too unproductive and too soft for market purposes. It ripens just after the May Duke. Fruit roundish, the skin thin and amber with mottled red.

Varieties Subject to Rot.—In general, it may be said that the tender-fleshed varieties of cherries rot worse than those having firmer flesh, and among those which are the most susceptible to it are Governor Wood, Elton, Coe's Transparent, Belle d'Orleans, Belle Magnifique, Rockport Bigarreau, Cleveland Bigarreau and Napoleon Bigarreau. Among those that are least susceptible are Black Tartarian, Black Eagle, Knight's Early Black, in the hearts, and Robert's Red Heart, Mezel, Tradescant's Black Heart and Windsor in the firm-fleshed varieties.

Family Sorts.—For the family varieties the hearts are among the best on account of their tender, luscious flesh, though in point of excellence, some of the bigarreaus are close competitors. I would recommend the following varieties for family use:

Hearts.—Black Tartarian, Governor Wood, Coe's Transparent, Belle d'Orleans, Downer's Late Red, Black Eagle, Knight's Early Black.

Bigarreaus.—Napoleon, Rockport, Robert's Red Heart, Yellow Spanish and Windsor.

Dukes.—May Duke, Belle de Choisy.

There are other varieties which are probably equally as good, but these can be recommended from long acquaintance.

Diseases.—The cherry is attacked by the same diseases that are common with plums, the principal one of importance affecting it being the fruit rot. The symptoms are familiar to all. The fruit turns brown and ash-colored tufts appear on it, which are the spore-bearing threads, and later the fruit falls, or becomes mummified and persists for a long time without decaying.

The twigs, leaves and flowers may also be attacked by the disease, causing the flowers to decay and die and the leaves to become discolored. The fungus passes the winter in the mummified fruits, and begins to propagate in the spring with the advent of warm weather. It attacks the fruit mainly just at the ripening period, principally during hot, muggy weather following a shower, or in a humid atmosphere, and the spread of the disease is so rapid that it may ruin an entire crop in twenty-four hours.

It is said by some that hand picking of the diseased fruit and parts will prevent the spread of the disease, as it destroys the means by which the fungus passes the winter. This method is not practicable, because not more than a part of the fruit will be gathered, and because of the labor attached to it.

It has been shown that spraying with Bordeaux mixture will control the difficulty and also prolong the ripening season.

The directions given by Craig are that the trees should be carefully sprayed with Bordeaux mixture after the blossoms fall and that two or three applications should be made, the last being with ammoniacal copper carbonate a few days before picking. The application should probably be made every two weeks. I should substitute the ammoniacal carbonate of copper for the Bordeaux after May 1, as traces of Bordeaux will remain on the trees for a month or more.

I am not convinced, however, that spraying is necessary to prevent the brown rot in New York State. The losses from this disease which have come under my observation are invariably the result of letting the fruit hang on the trees till ripe and then the rot is very active; but cherries should be picked a few days before ripe, before they soften, and then the rot does not seriously affect them. An illustration of this point, which is a most important one, was brought to my notice the present season. The last week of June, in eastern New York, was very hot and close, with showers every day or two. The cherries were then ripening and the conditions were favorable for the rot to spread. In one orchard from which several tons of cherries were shipped that week, there was not more than 150 pounds destroyed by the rot, while in another orchard a few miles distant at least 10 tons of the same varieties were ruined on the trees. In the first orchard the fruit was picked before it had ripened, and all that was fit was taken off as soon as the trees dried off after a shower; in the other orchard it was left till nearly ripe, and one-half to two-thirds of the crop was lost before the fruit could be picked. So rapid is the work of this fungus at this period, that the owner of the orchard told me that he lost 3 tons of one variety in one night. It might be added that the orchard first mentioned was a much stronger one, as it was in cultivation, while the last had been in sod for years, and the general debility and neglect of the trees made them good subjects for the attacks of rot or any other disease.

The most serious insect pests are the black aphis and curculio. The aphis often attacks young trees and sometimes the bearing ones. They appear early in the season and multiply very fast. This aphis is found in great numbers on the young shoots and the under side of leaves and on the stems of the fruit, excreting a sticky substance which covers the pests; and the leaves curl up. It may appear as late as September, but seldom in sufficient numbers to do injury. The aphis is a sucking insect and has to be treated with kerosene emulsion or whale oil soap, of which one or two thorough sprayings is generally sufficient to clear the trees. The spray must be applied as soon as the aphis appears, or the attacked leaves curl with the insect inside and it is impossible to reach them, and the full grown insect is very hard to kill. In such cases it is advised to pick the affected leaves and destroy them, if possible, and then spray so as to kill the remaining ones on the twigs and fruit.

The full treatment of the curculio is to be made the subject of a separate bulletin.

EPITOME.

Cherry growing is one of the neglected industries of western New York. There are practically no bearing orchards of sweet cherries, and very few of sour cherries.

The product is sold both in the open market and to canneries. In general, the factories afford the better market, although well grown and nicely packed fruits, particularly of the sweet kinds, find a ready sale in the general market.

Cherries like a loamy soil which is rich in mineral food. They should generally be given clean and frequent cultivation until the fruit is ripe, and after that the land may be put to rest with some cover crop. Stimulating or nitrogenous manures should be used cautiously.

Sour cherries should be planted 18 to 20 feet apart each way and sweet cherries about 10 feet farther.

Cherries are pruned after the manner of pruning plums and pears. Sweet cherries should be pruned to three to five main arms, and not to a central leader.

The curculio is the worst enemy to sweet cherries, and it is sometimes serious upon the sour kinds. Jarring the trees is the most reliable procedure.

The rot, due to a fungus, is particularly bad upon the early and soft-fleshed sweet cherries. Spray for it twice before the fruit is half grown, with Bordeaux mixture. Plant varieties least susceptible to the disease. Be expeditious in handling the crop.

Cherries for the general market should be carefully hand-picked, with the stems on, and they should be neatly packed in small packages. Cherries for the general market, particularly the sweet kinds, should be handled with as much care as strawberries are. The smallest packages are the most profitable for the best cherries.

The most deserving sour cherries for western New York are Montmorency, English Morello and Louis Philippe. The last is best in quality, but apparently is least productive.

Of sweet cherries, the following are recommended for market: Windsor, Napoleon, Black Tartarian, Black Eagle, Mezel, Robert's Red Heart, Downer's Late Red. For home use, Black Tartarian, Governor Wood, Coe's Transparent, Belle d'Orleans, Downer's Late Red, Black Eagle, Knight's Early Black, Napoleon, Rockport, Robert's Red Heart, Yellow Spanish, Windsor, May Duke, Belle de Choisy.

L. H. BAILEY,
G. H. POWELL.

[FROM SCIENCE PROGRESS.]

THE ZOOLOGICAL POSITION OF THE TRILOBITES.

THE veil of mystery which has so long hung over these early inhabitants of the earth has, within the last few months, been almost completely withdrawn. The apparently endless variations of the Trilobites, none departing far from the common type, have so long excited the admiration of the intelligent students of nature, and have given rise to so many speculations as to their relationships, that an account of the most recent discoveries can hardly fail to be welcome to a wide circle of readers.

The fact that only the dorsal surfaces of the Trilobites have been, as a rule, preserved, has always been especially tantalizing. While few natural objects have been so closely and exhaustively studied as the jointed dorsal shields of the Trilobites, their under surfaces have hitherto eluded even the most searching investigations. Beyond the great labrum, very little could be found which, with any certainty, could be ascribed to the under surface of the original animal. It is this long looked for under surface which has now been partially discovered in at least two species.

Although the new light has burst thus suddenly upon us, the patient researches of naturalists, like Henry Woodward and Walcott in recent times, not to mention the earlier workers, have not gone altogether unrewarded. Fragmentary glimpses of limbs, parts of limbs, and even parts of series of limbs have been brought to light. But so dense has been the mystery enveloping the group, so easy was it to find traces which might be limbs or might not, that all "discoveries" of this kind were looked upon with suspicion.

The limbs have now at last been discovered in a way which leaves little to be desired, and it is now possible to assign to the Trilobites their real position in the scale of animate nature with an accuracy for which, two years back, no one would have dared to hope. It must not, however, for a moment be supposed that we know all about the Trilobites—far from it. In spite of the new and splendid discoveries in America, our knowledge remains very fragmentary. But, among the additional facts which are now in our possession are just those which were most wanted by the morphologist to enable him to give the Trilobites their true place in the animal kingdom.

As I propose to pass these new facts in review and to discuss their bearings, a longer retrospect must be dispensed with. Suffice it to say that until comparatively recently zoologists, relying upon comparisons with existing groups, were generally agreed that the Trilobites were in some way related to the existing Crustaceans, but as to the nature of the affinity opinions greatly differed. In 1881, however, Professor Lankester* connected them with the Arachnids rather than with the Crustacea, basing his argument upon an elaborate comparison of Limulus and certain Eurypterids (both of which are undoubtedly related to the Trilobites) with Scorpio. This suggestion is still a matter of controversy, and a review of the arguments for and against would take us too far from our present subject. Further, the Trilobites, in spite of their prime importance in all discussions as to the affinity of the Merostomata, play only a passive part in the discussion, being dragged in, as if of small account, by their specialized offshoot Limulus. We may, however, at once state that the new evidence as to the position of the Trilobites is entirely in favor of the older view which linked them with the Crustacea.

Returning then to this older hypothesis, two views were sufficiently clearly defined to admit of discussion and criticism. One sought to ally the Trilobites with the Phyllopods, and relied chiefly upon the variability in the segmentation common to these two, and also upon the likeness between the head shield of Apus and of other extinct Phyllopods with the head shield of the Trilobites. The other saw in the Trilobites the ancestors of the modern Isopods, and relied chiefly upon the great external resemblance existing between the two forms. Neither case was really very strong. The absence in the Phyllopods of anything like the pleuræ of the Trilobites, and the difference between the leaf-like limbs of the former and the filamentous limbs attributed to the latter, were acknowledged difficulties against the former supposition. While on the other hand, the fixed segmentation of the Isopods and their uniform division into well defined head, thoracic, and abdominal regions showed them to be far more highly organized than the Trilobites. Lastly, against both these affinities the total absence of any traces of antennæ in the Trilobites served as a warning against any too rash assertion of their Crustacean affinities.

So far, however, as this absence of the antennæ in the Trilobites could tell us anything, the advocates of the Phyllopodan affinity were better off than the advocates of the Isopodan affinity. In the Phyllopod Apus, the antennæ are not visible from the dorsal surface, being ventrally placed. If the antennæ in the Trilobites were like those of Apus, they would have vanished with the rest of the ventral appendages. Their absence in the Trilobites therefore is just what their assumed relationship with Apus would lead us to expect. On the other hand, the same fact was a serious difficulty against the Isopod relationship, as these have very pronounced, anteriorly placed antennæ, which made it difficult to understand how they could be completely absent in the Trilobites, if there were any genetic relationship between them.

In 1892 the present writer made a detailed study of the morphology of the Phyllopod Apus, which led him to the conclusion that it was the most primitive of all existing Crustacea, and was, indeed, an ideal transition form between the chaetopod Annelids and the higher Crustacea. Such a conclusion necessarily led to a comparison between Apus and the earliest known fossils which had any claim to be considered Crustacea, viz., the Trilobites. Several new points of resemblance were discovered, of which one was of prime morphological importance. The most startling feature in the organization of Apus was shown to occur also in the Trilobites.

The peculiarity referred to is the remarkable character of the system of appendages in Apus. They commence anteriorly, one well developed pair on each segment. But from before backward they progressively diminish in size, and about half way along the trunk commence to be more and more crowded. The most posterior pairs are quite microscopical, and so crowded are they that as many as six pairs appear to occur on one and the same body segment. There are occasionally as many as sixty to sixty-five pairs of appendages in all! This phenomenon is quite unique among existing Arthropods, and had long appeared inexplicable. The explanation offered by the present writer was briefly as follows:

Apus, as the most primitive of all extant Crustaceans, had for its immediate ancestors animals with some sixty to seventy developed segments. In Apus these sixty or more inherited segments commence to appear, but the bulk of them remain rudimentary. We thus have the most developed limbs anteriorly, while posteriorly they decrease in size until they are little more than closely crowded limb buds. Each pair of limbs, therefore, represents a true segment. In the posterior trunk region of Apus, where more than one pair of limbs occurs on a body ring, this latter is not a true segment, but a secondarily formed segment, consisting morphologically of as many true segments as there are pairs of limbs developing from it.

This explanation was strongly confirmed by an examination of the internal organization of Apus, in which the progressive rudimentation from front to back which it postulated could be clearly traced, notably in the arrangement of the musculature in the heart, and in the genital glands. On the other hand, that each limb bud represented a true segment seemed to be established by the presence of a distinct ventral ganglion for each pair. Lastly, an appeal to the developmental history left little doubt as to the correctness of our interpretation of this long standing enigma. Allowing for special modifications of the anterior functional appendages of the Nauplius, it is, at any stage, merely a small Apus showing the same phenomenon. The anterior functional limbs are followed by a series of gradually diminishing rudimentary appendages. The fully developed Apus is, in fact, little else than an enlarged, not metamorphosed, Apus Nauplius. At any stage at which the growth stopped there would always be a number of progressively diminishing limb buds and undeveloped segments. We accordingly find in the different species of Apus a very varying number of these rudimentary limbs behind a certain number of well developed appendages at the anterior end of the body.

This argument is still further strengthened when the character of the limbs is also taken into account. A distinct tendency toward the formation of jointed filamentous appendages can be seen at the anterior end of the trunk. This is gradually lost toward the posterior end, and the limbs become more and more leaflike and primitive, i. e., they are flat, unjointed skin folds.

Here then was an immense amount of evidence to show that the immediate ancestors of Apus must have had upward of sixty well developed segments. Apus then must stand at the very bottom of the genealogical tree of the modern Crustacea, with their nearly constant number of segments, twenty to twenty-one. Indeed, as above stated, so low did Apus appear in the scale of Crustacean organization that it seemed to form a true connecting link between the Crustacea and the Annelids.

Now this same remarkable character in the segmentation, its fixation at the posterior end of the body in a rudimentary condition, which is unknown in any other living Crustacean besides Apus, is visible in many Trilobites. The segments taper away at the end of the body, decreasing progressively in size. So feebly developed, indeed, are the most posterior segments, that I am disposed to consider the pygidium of the Trilobites as a modification largely due to the difficulty of bending such rudimentary segments one upon the other. The earliest known genus, Olenellus, has the diminishing segments free to the end, and is, in this respect, even more primitive than Apus. The free condition of the segments in Olenellus shows that the pygidium, which sometimes exhibits no traces of segmentation, was in fact a secondary modification.

* "Limulus an Arachnid," Q. J. M. S., vol. xxi.

But this was not all; in addition to the visible posterior tapering away of the segments, Walcott's* hypothetical restoration of the under surface, based upon his own and previous discoveries, showed the limbs diminishing in size toward the posterior end of the body, as they do in Apus. There was then no escape from the conclusion that the Trilobites were, like Apus, derivatives of forms with still richer segmentation than they themselves possessed, and that they, like Apus, were shortening their bodies by the fixation of a number of the posterior segments in a rudimentary or larval condition.

When, therefore, we added to this the striking resemblances between their head shields, the similarity in form and position of the large labrum, the presence in both of an intestinal tube bent sharply round anteriorly and ventrally so as to open backward above the labrum, the association of Apus with the Trilobites ceased to be a matter of speculation, and it was safe to predict that when actual discovery revealed to us more details as to Trilobite structure, organs would be found homologous with, and probably closely resembling, those of Apus. For instance, a pair of minute cirrus-like antennæ could safely be assumed to have existed ventrally on each side of the labrum of the Trilobites, and thus not visible from the dorsal surface.

There was only one objection which might perhaps have been raised against this association of Apus with the Trilobites, though hardly by a morphologist. The limbs of Apus are phyllopodan, i. e., flat, leaflike, swimming appendages, while those of the Trilobites, as far as the evidence went, were filamentous and, according to Walcott's restoration, filamentous even in the rudimentary pygidial segments. This difficulty, by no means insuperable, has, as we shall presently see, been completely disposed of by the recent discoveries.

The first here to be recorded is not one of the most recent. I refer to Walcott's discovery of the fossil Protocaris Marshii, which shows that Apus-like Crustacea were contemporaneous with the very oldest known Trilobites.

Of the new discoveries which have thrown so much light on the details of organization of the Trilobites, the first was in August, 1893, when Mr. W. D. Matthew announced the discovery of specimens of Triarthrus Beckii, showing clear traces of antennæ and of other limbs.† The first specimen was discovered by Mr. W. S. Valiant, as long ago as 1884, in the Utica slate, but it was not until 1892 that he found other specimens near Rome, New York, which put the matter beyond question. So fine grained is the matrix, that not only the limbs, but the setæ on the limbs have left distinct impressions. This discovery, so long waited for, naturally riveted the attention of those who had studied the subject at all.

It is, however, the name of Dr. C. E. Beecher which in the future will be most intimately associated with this advance in our knowledge, owing to his patient unraveling of the finer details of structure and organization, as these could be gathered from a comparison of many specimens. In a series of papers he has described, in addition to the antennæ, the limbs of the thorax, the limbs of the pygidium, the limbs of the head, with the metastoma or under lip of Triarthrus, and besides these, some of the posterior limbs of Trinucleus, including those of the pygidium.

I propose here to discuss a few of the more important of these new details, and to show how they reveal the true position of the Trilobites in the genealogical tree of the Crustacea.

We owe the discovery of the antennæ and limbs of Triarthrus chiefly to the fact that in that form these appendages were so well developed as to project all around beyond the edge of the dorsal shield. It is a common phenomenon in the animal kingdom to have one member of a group specialized by the great length of the appendages, all lengthening alike, the antennæ as well as the legs. The specimens of Triarthrus, then, were found with a pair of cirriform antennæ projecting from under the head shield, and a confused fringe of appendages protruding along each side to the posterior end of the body, from under the dorsal shield.

The fact that the antennæ came from under the head shield was important, as the antennæ of Apus are attached under the head shield on each side of the prostomium or large upper lip, whereas, as a rule, among the higher Crustacea, the antennæ are placed quite anteriorly. And as a matter of fact, examination of fortunate specimens showed that the antennæ were inserted in Triarthrus in practically the same relative position to the prostomium as they are in Apus.

If verifications of predictions based upon a theory are proof of the correctness of that theory, here was the proof of the relationship between Apus and the Trilobites. That relationship had been based upon other resemblances of organization, some of the more important of which have been briefly indicated above, and little more than a year after the antennæ of the Trilobites were found exactly where it was predicted they would be found. The importance of this point will be understood when it is remembered that no other known Crustaceans, recent or fossil, except Apus and the Trilobites, possess the great square labrum pointing backward, with the antennæ inserted on each side of it, the whole hidden under a great head shield.

Before passing on to the next point this ventral position of the antennæ and labrum deserves attention. Occurring in Apus with its traces of sixty segments, and in the Trilobites, the oldest fossil Crustaceans, no one can doubt that it is the primitive position of these organs in the crustacean phylum. It follows, therefore, that the annelidan ancestors of the Crustacea, with their originally anteriorly placed prostomium, must have bent the first segment sharply round in order to bring that organ into the ventral position found in the primitive Crustacea. There is, in fact, abundance of evidence to show that such a bending did take place.

Without going into details which have been elaborated elsewhere, I may mention a few points, e. g., the sharp anterior bend in the intestinal canal; the curious clumping of the ventral longitudinal muscles, which form the "cartilaginous" endosternites in the angle of the bend; the corresponding stretching out of the longitudinal muscles on the dorsal side; the simple explanation this bending round of head segment affords of the translocation of two pairs of limbs, originally post-oral, into the pre-oral position occupied by the antennæ in the Crustacea; and lastly, the first ontogenetic appearance of the eyes in Apus in a more anterior and ventral position than they possess in the adult, and their subsequent wandering upward on to the dorsal surface.

(To be continued.)

* "The Trilobite: New and Old Evidence Relating to its Organization." Bull. Mus. Comp. Zool., vol. viii, 1880-81.

† "On the Antennæ and other Appendages of Triarthrus Beckii." Amer. Jour. Sci., ser. 3, vol. xlvi, p. 131.

THE FILEFISH.

THE filefish is found along our Atlantic coast from Cape Cod to Florida and in the waters of the West Indies; it is common here in summer; there are eleven filefish now at the New York Aquarium that were taken in Gravesend Bay. When motionless or when moving about slowly, the filefish ordinarily carries itself with its head downward, at an angle of forty-five degrees or more. Often it takes a position more nearly horizontal; and often head downward, a position almost, if not quite, perpendicular; sometimes, indeed, it inclines backward. The filefish, which is not by any means the only fish that has in this way the appearance of standing more or less on its head, takes these attitudes probably for greater convenience in feeding; when swimming more rapidly or going a distance it moves with the body horizontal, in the usual manner.

The filefish feeds on small crustaceans, including barnacles, and such other growths of animal life as it finds on rocks, on stone work and spiles, and on marine plants; it eats shrimps when it can catch them, and it picks up small scraps of food, fragments left by larger fishes in feeding. The filefish is very rarely eaten; it is but little more than skin and bones. It can be dried in the sun. Fishermen sometimes dry the filefish, give it a coat of shellac to preserve it, and then gild it for an ornament. It shrivels somewhat in drying, and there is so little flesh on it that it shows the bony structure.

In color and in form the filefish varies extremely with age. Its first dorsal fin consists of a single spine, which, in the mature fish, is long, slender, rough and file-like. It is from this spine and from the rough, file-like character of its skin that the filefish takes its name; there is, however, a West Indian filefish that has a smooth skin. The filefish has a pointed snout, its body is thin and deep, and the young fish has a rather long, slender and pointed tail; in the adult fish the tail is rounded, and it becomes proportionately shorter with age. The mouth is very small and obliquely placed; the under jaw projects; the lower teeth are directed obliquely upward and backward. The eye is large. The first dorsal fin or file-like spine is carried lying down against the back or erected at one angle or another. The soft dorsal fin and the anal fin are very long at the base, and each is composed of a large number of rather short rays; when the fish swims these fins have a wavy motion.

In the change of form which it undergoes the filefish is remarkable. Not only is the tail much longer and more pointed in the young fish, but in the very young, one, for example, not more than three months old, that portion of the body in front of the soft dorsal and of the anal fin is twice as deep as the part to which the dorsal and anal fins are attached; the fish widens on the under side, the wider part extending down below the rest of the fish's body abruptly, something as the after edge of a centerboard extends below the keel of a boat.

When the fish is a year old this abrupt break in the line of the under side of its body will have entirely disappeared. In the fish of three months of age, which would be about three and a half inches long, the dorsal spine would be rudimentary, a tiny spike an eighth of an inch in length. In a fish eight or ten inches in length and a year old the dorsal fin or spine would have attained a length of about two inches.

The very young fish is distinctly banded on both body and tail; the older fish, which shows no trace of bands, is mottled; the oldest is nearly uniform in color. The oldest is called, on account of its color, the orange filefish. The orange filefish grows to be eighteen inches or more in length.

Among the filefishes at the aquarium there is one orange filefish; it is sixteen or seventeen inches long. The greater part of its body is orange colored: the rest, including a portion of its head and of the forward part of its body on the under side, is of a drab or lead tint. The other ten filefishes at the aquarium, which range in size from six to twelve inches, are all mottled; they are of a dark chocolate or dark brown and grayish white. The mottling varies somewhat in shade in different fishes, being lighter in some and darker in others, and it may also vary in shade at different times in individual fishes.—New York Sun.

BLOOD EXAMINATION IN DISEASE.

UNDER this heading, in a recent number of Science, Prof. Joseph F. James, of Washington, D. C., comments upon an assertion of Prof. John Michels given in SUPPLEMENT to the effect that "no physician ever makes a microscopical examination of blood in making his diagnosis."

Prof. James says: "That there still remains much to be learned regarding the blood is undeniable. But that the medical profession is in a state of ignorance in regard to it, or that no one ever makes a microscopical examination of blood in making his diagnosis, is absolutely false. Since the discovery of the hematozoa of malaria by Laveran, in 1880, thousands of cases of malarial fever have been diagnosed absolutely by blood examination. All late books on the practice of medicine refer to this as a valuable aid to diagnosis in this disease. Dr. William Osler, of Johns Hopkins University, who has made a special study of malarial diseases, can, perhaps, give Prof. Michels some information on this point.

"So, too, in cases of anæmia. An examination of the blood will infallibly diagnose the case, and all physicians in cases of doubt make this examination or have it made. Special instruments like the hæmacytometer of Gowers or Thoma, or the hæmaglobinometer of Gowers, have been made for this purpose and can be purchased from all dealers in microscopical instruments.

"The disease known as filariasis can be and is diagnosed by blood examination. The parasites causing this disease occur in the immature state in the blood, passing, as they mature, into the lymphatics. These parasites are truly remarkable from the fact that they are found in the blood only at night, being almost or entirely absent in the daytime; if, however, the patient sleep during the day, this is reversed, thus showing that the condition of sleep is an important factor in determining the presence of the organisms.

"From these facts it would seem that the medical profession is not in quite as 'dense' a state of ignorance regarding the blood as Prof. Michels would have his readers believe, and that they do make use of blood examination in the diagnosis of disease."

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